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AVAILABLE ENERGY[1]

ASTRONOMY is often called the most useless of the sciences, and so it is from the standpoint of the man whose time-horizon extends ten years forward and ten years backward; and that man, too, probably represents ninety per cent. of all mankind. But for the smaller fraction of men who have been able to rise above the mole's outlook, who have studied enough of the past and understood enough of the present to have acquired a three hundred year time-horizon, for all such the foregoing statement is grotesquely incorrect.

If utility consists in nothing more than feeding and clothing the generation now living, then there are indeed *useless* sciences, and astronomy is perhaps one of them. But again, if utility consists in nothing more than feeding and clothing ten successive generations, then, even by that narrow standard, the verdict of history has definitely been that astronomy is one of the most useful of all the sciences, more useful probably than physics, chemistry, geology or engineering, and that for the simple reason that without it there would presumably never have been any modern physics, chemistry, geology or engineering. Eliminate it and you probably eliminate with it the development of the whole of Galilean and Newtonian mechanics: you certainly eliminate the discovery of the law of gravity, and of all the principles of *celestial* mechanics, and you probably eliminate even the discovery of the laws of force and motion. All these discoveries which came out of the labor and travail of two long centuries, the seventeenth and the eighteenth, which had to create even a new mathematics in order to be able to handle the new group of physical ideas, constituted an indispensable foundation for the crowning achievement of the nineteenth century, namely, the *application of these same laws to the development of terrestrial mechanics,* an achievement out of which has grown most, if not all, of the *distinctive* features of *modern* civilization.

Utility can only be properly defined as all that contributes to the finer, fuller, richer, wiser, more satisfying living of the race as a whole, and there is scarcely a bit of knowledge of the external world or of man himself that does not definitely help toward

[1] Address before the Society of Chemical Industry, New York, September 4, 1928, on the occasion of the conferring on Dr. Robert A. Millikan of the Messel Medal in honor of his work on the structure and relations of atoms.

that end. Some of the most useful discoveries have exerted their chief influence, not through showing how the yield of beans or of cabbages per acre could be doubled, but rather through preventing mankind from wasting its precious energies on useless undertakings, such for example as building a tower of Babel. All knowledge that helps toward an understanding of the nature of the universe of which we are a part is useful, for we need very much more of it than we now have, or shall have for centuries to come, to enable us to direct our energies toward wise, effective living instead of wasting them on beating tom-toms, inventing perpetual-motion machines, or chasing either physical or social rainbows. "A penny saved is a penny earned," and this is quite as true of human energies as of household economics.

The disasters that can befall mankind merely because of erroneous conceptions of the nature of the world in which we live are well illustrated by the historic record of the miseries that came upon the earth in the year 1000 A. D. because of the widespread belief that the world was coming to an end at that time. The recent exact measurement of the amount of lead in Black Hills uraninite, and of the exact atomic weight of that lead, is not usually regarded as a great engineering undertaking, nor as an accomplishment fraught with important useful consequences; but I venture the estimate that the knowledge that has come from that and similar experiments to the effect that this world has already had a lifetime of at least a billion years, and that man has in all probability another billion years ahead of him, in which there is the possibility of his learning to live at least "a million times more wisely" than he now lives, is likely to have in the long run a much larger influence upon human conduct than the invention either of the airplane or of the radio, important and preeminently useful though these be. Similarly, the discovery of the second law of thermodynamics has been perhaps more useful in preventing useless effort, in spite of the legions of perpetual motion cranks who still infest all physical laboratories, than in improving the efficiency of the heat engine.

The foregoing considerations constitute my argument for the appropriateness of presenting before the Society of Chemical Industry some discoveries that have not heretofore been labelled "useful" but which, when put together and correctly interpreted, as I hope they are herewith, may constitute an important and perpetually burning beacon to point out to mankind the way of progress, even of *industrial* progress.

The four recent developments in the field of pure science that I am herewith endeavoring to fuse together into a result of industrial importance are: (1) the discovery of the relation between mass and energy; (2) the development of methods of making very exact atomic-weight determinations; (3) the discovery of the cosmic rays; (4) the development of relativity—quantum mechanics. It may appear at first sight to the average supporter of Al Smith that I have taken a large order, but pray withhold your judgment.

The first of these four very recent developments was stated in its general equational form, namely, $mc^2 = E$, in which m represents mass in grams, c the velocity of light, and E energy in ergs, by Einstein as one of the most important consequences of the special theory of relativity (1905); but it had been experimentally established for special cases before Einstein's day, namely, in 1901 and 1902 by Kaufmann's measurements on the variation of the mass of the electron with its kinetic energy, and it had also been made by Lorentz a theoretical consequence of the electro-magnetic theory of the origin of mass. This equation may therefore, I think, be taken as a safe practical guide even by those who hope that the special theory of relativity may ultimately be found to involve some second-order error. The Michelson-Morley experiment has surely been sufficiently checked to establish the fact that it involves no first-order uncertainty.

The second of the foregoing developments is due primarily to Aston, of Cambridge University, England, who last summer, 1927, established experimentally a definite relation represented by a smooth curve, between the atomic weights of the elements and the mass of the positive electron as it appears in the constitution of the nucleus of each particular atom. This relation is a purely empirical one, the bearing of which upon the argument herewith presented has never, so far as I know, been pointed out before, at least in a quantitative way, save in very recent papers by Dr. Cameron and myself, the most complete and important of which is to appear in the October number of *The Physical Review*.

The point upon which we lay emphasis is that if this smooth experimental curve may be taken as a safe guide, then by combining it with Einstein's equation we can at once draw very important conclusions about the possible sources of available energy.

The first conclusion that we draw is that *the process of radioactive disintegration with the ejection of an alpha particle is a process that can take place only in the case of a very few of the very heavy and very rare elements.* For radioactivity is a heat-evolving, *i.e.*, an exothermic process, otherwise it of course could not take place of its own accord, and Einstein's equation tells us that no energy-evolving or exothermic process can take place unless the total mass of all the constituents after the change is less than the total mass before the change—that an equivalent mass must

always disappear if other forms of energy are to appear. But the relation of these masses before and after any hypothetical change is just what is given by Aston's curve, which shows that no element of atomic weight under say about 100 can disintegrate with the ejection of an alpha particle and the evolution of energy. And yet more than ninety-nine per cent. of all matter consists of these atoms of atomic weight less than 100. Therefore radioactivity with the ejection of alpha particles, even of a very feeble energy, is not a general property of matter, as many of us have in the past thought it might be. Under the stimulus of the discovery of the enormous quantities of energy evolved in the disintegration of uranium and thorium we have often imagined, and sometimes incautiously stated, that there might be similar amounts of available energy locked up in the common elements, releasable, perchance, by getting them to disintegrate, as uranium and thorium spontaneously are doing. And engineers, physicists and laymen alike have talked glibly about "utilizing this source of energy when the coal is gone." So-called humanists, on the other hand, advocates of a return to the "glories" of a pre-scientific age, have pictured the diabolical scientist tinkering heedlessly, like the bad small boy, with these enormous stores of sub-atomic energy, and some sad day touching off the fuse and blowing our comfortable little globe to smithereens.

But Nature, or God, whichever term you prefer, was not unconscious of the wisdom of introducing a few fool-proof features into the machine. If Einstein's equation and Aston's curve are even roughly correct, as I am sure they are, for Dr. Cameron and I have computed with their aid the maximum energy evolved in radioactive change and found it to check well with observation, then this supposition of an energy evolution through the disintegration of the common elements is from the one point of view a childish Utopian dream, and from the other a foolish bugaboo. *For the great majority of the elements, such as constitute the bulk of our world, are in their state of maximum stability already.* They have no energy to give up in the disintegrating process. They can only be broken apart by working upon them, or by supplying energy to them. Man can probably learn to disintegrate them, but he will always do it "by the sweat of his brow."

But having thus disposed of the process of atomic disintegration, and found it completely wanting as a source of available energy, since the radioactive elements are necessarily negligible in quantity, let us next see what there is to be learned about the process of atom-building as a source of energy. Here Einstein's equation, Aston's curve, and the third of the foregoing developments in pure science, namely, the recent experimental work on cosmic rays, have just thrown a flood of light on the processes going on in this universe in which we live. For, first, Dr. Cameron and I have recently found three definite cosmic ray bands, or frequencies, of penetrating powers, or ray-energies, respectively, twelve, fifty and one hundred times the maximum possible energies that are, or can be, obtained from any radioactive, that is, any disintegrating, process. The highest frequency band has so enormous a penetrating power that it passes through as much as 200 feet of water or eighteen feet of lead before becoming completely absorbed, while two or three inches of lead absorbs the hardest gamma rays. This discovery of a banded structure in cosmic rays shows that these rays are not produced, as are X-rays, by the impact upon the atoms of matter of electrons that have acquired large velocities by falling through powerful electrical fields, as we earlier suggested—the fields needed to produce frequencies as high as those of the highest observed cosmic rays are equivalent to 216,000,000 volts—but that *they are rather produced by definite and continually recurring atomic transformations involving very much greater energy-changes than any occurring in radioactive processes.*

Taking Einstein's equation and Aston's curve as a guide there are no possible atomic transformations capable of yielding rays of the enormous penetrating power observed by us, except those corresponding to the building up or creation of the abundant elements like helium, oxygen, silicon, and iron out of hydrogen, or possibly in the case of the last two elements out of helium. The entire annihilation of hydrogen by the falling completely together of its positive and negative electrons has been suggested as an additional possibility, but it can be eliminated in this case for two excellent reasons. The first is that there is no place for such a radiation to occupy in the observed cosmic-ray curve; and the second is that this radiation, if it were present, would necessarily be homogeneous and could not by any possibility exhibit the banded structure shown by the observed cosmic rays. So that here alone, by a process of exclusion, we have arrived at pretty definite evidence that *the observed cosmic rays are the signals broadcasted throughout the heavens of the births of the common elements out of positive and negative electrons.*

But right here is where the fourth of the aforementioned recent developments in the purest of pure science dovetails into the practical picture. Dirac is a young Englishman deeply versed in all that the engineer of to-day—and even the experimental physicist too—is rather proud to say "is altogether beyond him," meaning thereby that he considers that there are more important things for him to put his energies upon.

Such highbrow subjects as relativity-quantum mechanics, and the new wave-mechanics, to which Dirac has made outstanding contributions—where do they touch life anyway? Very quickly has come the answer. Dr. Cameron and I had measured fairly accurately the penetrating powers, or absorption coefficients, of our three prominent cosmic-ray bands. Without being guided by any theory at all we had found them at $\mu = 0.35$, $\mu = 0.08$, $\mu = 0.04$ where μ means absorption coefficient per meter of water. Dirac's relativity-quantum-mechanics formula, giving the quantitative relation between absorption coefficient and frequency, or energy, this formula being an extension of and correction to one originally worked out with consummate skill by Arthur H. Compton, of Chicago, last year's Nobel-prize winner in physics, enables us to compute accurately from Einstein's equation and Aston's curve what should be the absorption coefficients, or the penetrating powers, of ether-waves produced by the act of creation of the common elements out of the primordial positive and negative electrons.

Before presenting these computations, however, let me build a little more background. It is an interesting and a very important fact from the practical viewpoint, too, that more than ninety-five per cent. of this universe, so far as we can now see, is made up of a very few elements.

First. The spectroscopy of the heavens shows the enormous prevalence everywhere of hydrogen, but hydrogen is merely the primordial positive and negative electrons tied together, or in process of being so tied.

Second. The spectroscopy of the heavens also shows that helium is an exceptionally abundant, and a very widely distributed, element, even though, because of its lightness and inability to combine with anything, even with itself, the earth has not retained much of it. Significant is it, however, that the alpha particle given off by all the heavy radioactive elements is nothing but helium, so that it must have a certain prevalence even on earth in the structure of the heavier elements.

Third. Dr. Bowen, at the California Institute, has just shown that "nebulium," also abundant almost everywhere in the heavens, is nothing but oxygen and nitrogen, while oxygen alone constitutes 55 per cent. of the earth's crust, and about the same proportion of meteorites. Oxygen and nitrogen, then, which for our present purpose will be treated as one element, since they have nearly the same atomic weight and will be henceforth listed under the name of the stronger brother, constitute the third extraordinarily abundant element, and it is to be noted that there are no abundant elements at all between helium and oxygen. Carbon has a certain minor prevalence, but because of its nearness in atomic weight to nitrogen and oxygen it may here be treated as merely a feeble satellite to oxygen.

Fourth. Ninety-five per cent. of the weight of all meteorites consists of oxygen (54 per cent.), magnesium (13 per cent.), silicon (15 per cent.) and iron (13 per cent.), while 76 per cent. of the earth's crust is composed of the three elements, oxygen (55 per cent.), silicon (16 per cent.) and aluminum (5 per cent.), no other element rising over 2 per cent. Iron constitutes 1.5 per cent. of the crust, but it is supposed to be very largely represented in the interior. Because of the closeness in their atomic weights magnesium, aluminum and silicon (24, 27, 28) may, for our present purpose, be regarded as a single element and given the name of the strongest brother, silicon. There are then no abundant elements whatever between oxygen and silicon, nor between silicon and iron (atomic weight 56), *and from an engineering standpoint the universe may be said to be made up of the primordial positive and negative electrons, and of four elements built out of them, namely, helium, oxygen, silicon and iron.*

Let me now digress from my subject, "Available Energy," just long enough to point out the practical significance of the foregoing facts. Mankind, if he is here a billion years hence, will be satisfying his main needs, as he satisfies them now, with the four elements, hydrogen, oxygen, silicon and iron, *i.e.*, with water, air, earth and Fe, where the last symbol stands for iron rather than for fire, which was the fourth constituent of the world of the ancients. These fundamental facts may some time help to stabilize the stock market. Aluminum might some day compete with iron in usefulness if lightness were a desideratum, but for the great bulk of structural purposes it is not. The progress of science and invention is not likely to put out of business for a billion years to come the concerns engaged in the iron and steel industry.

But my subject to-day is not available materials but rather available energy. Einstein's equation and Aston's curve, then, enabled Dr. Cameron and myself to compute the energies sent out in the ether signals arising from the creation in single isolated acts of helium, oxygen, silicon and iron, and then Dirac's formula enabled us to compute the absorption coefficients of the corresponding cosmic rays. The theoretical values of the absorption coefficients corresponding to the first three of these creative acts came out $\mu = 0.30$, $\mu = 0.08$, $\mu = 0.04$, as compared with the previously obtained and already reported observed-values $\mu = 0.35$, $\mu = 0.08$, $\mu = 0.04$. The agreement is better than our observational uncertainty, and leaves no doubt whatever in our own minds that *the observed cosmic rays are in fact the birth-cries of the infant*

atoms of helium, oxygen and silicon. We have some little indications that we can also hear the shriller birth squeaks of infant iron, but we are not yet ready definitely to assert it.

But the question that is already being asked on all hands is "Where are these atom-building processes going on?" To this question, too, we think we have the answer. It is "not at all in the stars," for high temperatures and densities seem to be inimical at least to the process of the creation of the foregoing abundant elements out of the primordial positive and negative electrons. The building of the radioactive elements, which is an endothermic, or energy absorbing, process, may possibly be taking place in the stars where surplus energy is available for it. We have no experimental evidence whatever on this point. But we have what we consider excellent experimental proof that the foregoing endothermic processes that produce the cosmic rays do not take place in the stars at all. The full argument is given in the October issue of *The Physical Review,* but the fact that the sun, the great hot mass just "off our bows," has no influence whatever upon the intensity of the observed cosmic rays, for these come in just as strong at midnight as at noon, is enough to show that this particular star is not a source of cosmic rays. Since, however, these rays do come in to us all the time, and practically uniformly from all directions, Dr. Cameron and I can find no escape from the conclusion that these atom-building processes which give rise to the observed cosmic rays are favored by, and actually have their source in, the places in the universe where the temperatures and pressures are extreme in the opposite sense, *i.e.,* where they are close to absolute zero. In other words, *we think that the atom-building processes that give rise to the observed cosmic rays can take place only under the extreme conditions of temperature and pressure existing in interstellar, or intergalactic,* space.

Now combine this conclusion with that already arrived at by astronomers like Eddington and Jeans, who can find no way of accounting for the immense quantities of energy which for billions of years have been poured out by the sun and other stars, save in the assumption that under the conditions of stupendous temperatures and pressures existing at or near their centers mass is being wholly converted into radiant energy by the compiete falling together of positive and negative electrons. If this is a correct conception, and it has become orthodox astronomy, then the combination of it with the cosmic-ray evidence herewith presented *leads to the picture of a continuous atom-destroying process taking place under the extreme conditions existing in the interior of stars, and an atom-creating process continually taking place under the equally extreme conditions of just the opposite sort existing in interstellar space.* Let us analyze a little further these two processes.

The process in which positive and negative electrons under the influence of the stupendous temperatures and pressures existing in the interiors of stars completely fall together—this need happen only occasionally in the interior of very heavy atoms, heavier, Jeans thinks, than any existing on the earth—must in any case, as we find from Einstein's equation, be one that gives rise to an ether-wave about four times as penetrating as the most penetrating cosmic ray thus far observed. We have looked diligently for a cosmic ray of this sort, but it definitely does not appear in our cosmic-ray curves. This is, however, to be expected, since, according to its sponsors, it is formed only in the interior of stars, and hence is hidden away behind an impenetrable screen of matter that completely transforms it into heat before it gets out. Indeed, in accordance with the Jeans-Eddington theory this is merely the way the furnaces of the stars are continually being stoked and all that we ought to observe is the heat and light that they radiate in consequence.

The continuous formation, however, of the common elements in interstellar space newly and directly observed through the cosmic rays thereby sent forth raises imperiously the question as to why the primordial positive and negative electrons, which are the building stones of these common atoms, have not long ago been used up, since the process has undoubtedly been going on for eons upon eons. And the answer that Dr. Cameron and I wish to make is that out in the depths of space, where we actually observe, through the cosmic rays, helium, oxygen and silicon being continually formed out of positive and negative electrons, there too these positive and negative electrons are also being continually replenished through the conversion back into them, under the conditions of zero temperatures and densities existing there, of the radiation continually pouring out into space from the stars. With the aid of this assumption we are able to regard the universe as in a steady state now, and we are able also to banish forever the nihilistic doctrine of its ultimate "heat-death."

We regard our assumption as the least radical and the most satisfactory of any of the three between which, in any case, a choice must be made.

1. The first of these is that of Jeans,[2] that mass, *i.e.,* the electron (positive and negative alike) is convertible into radiant energy, *but that the process is nowhere reversible.* Matter will thus ultimately be all converted into radiation, *i.e.,* it will simply disappear. A recent statement of Jeans' reads: "Thus observation

[2] J. H. Jeans, Nature, *121,* 467, 1928.

and theory agree in indicating that the universe is melting away into radiation. Our position is that of polar bears on an iceberg that has broken loose from its ice pack surrounding the pole, and is inexorably melting away as the iceberg drifts to warmer latitudes and ultimate extinction."

This is the old hypothesis of the "heat-death." It conflicts with no observed facts, and before the advent of Einstein it was a necessary consequence of the Second Law *provided the universe were treated as a closed system.* Scientists, however, have always objected that such treatment represents an extravagant and illegitimate extrapolation from our very limited mundane experience and modern philosophers and theologians have also objected on the ground that it overthrows the doctrine of Immanence and requires a return to the middle-age assumption of a *Deus ex machina.* Since the advent of Einstein it meets the further difficulty that it injects into modern thermodynamics one single process—the convertibility of mass into radiant energy—which violates the principle of "microscopic reversibility" required by the modern statement of the Second Law.

2. The second possible hypothesis is that of Stern,[3] Tolman[4] and Zwicky,[5] that the foregoing processes are all everywhere reversible. This hypothesis keeps the second law intact, including microscopic reversibility denied by Jeans' assumption, but so far as we can now see it does not avoid the "heat-death," and it is not favored by the evidence herewith presented that the atom-building processes that give rise to the cosmic rays do not seem to be taking place everywhere, *e.g.,* in the stars, but do seem to be taking place solely in the depths of space.

3. The third hypothesis—that herewith presented—is just as radical as 1, but no more so, in denying microscopic reversibility, but it provides an escape sought in vain by both 1 and 2 from the "heat-death." Also it is just as radical as 2, but no more so, in assuming that radiant energy can condense into atoms somewhere, but it is in better accord with the cosmic-ray evidence that the atom-creating processes seem to take place only in interstellar space.

But if the point of view developed in the foregoing is correct what sources of energy are there, then, for man to draw upon during the next billion years of his existence? The answer has already been given but it may be restated thus:

(1) The energy available to him through the *disintegration* of radioactive, or any other, atoms may perhaps be sufficient to keep the corner peanut and popcorn man going, on a few street corners in our larger towns, for a long time yet to come, but that is all.

(2) The energy available to him through the *building-up* of the common elements out of the enormous quantities of hydrogen existing in the waters of the earth would be practically unlimited provided such atom-building processes could be made to take place on the earth. But the indications of the cosmic rays are that these atom-building processes can take place only under the conditions of temperature and pressure existing in interstellar space. Hence there is not even a remote likelihood that man can ever tap this source of energy at all. The hydrogen of the oceans is not likely to ever be converted by man into helium, oxygen, silicon or iron.

(3) The energy supplied to man in the past has been obtained wholly from the sun, and a billion years hence he will still, I think, be supplying all his needs for light, and warmth, and power entirely from the sun. How best to utilize solar energy it is not the purpose of this paper to reveal. That subject is treated in masterly fashion in a paper by Edwin E. Slosson entitled "The Coming of the New Age of Coal," printed in the *Proceedings* of the International Conference on Bituminous Coal held from November 15 to 18, 1926—a paper to which I refer the reader for the next chapter on "Available Energy." The present paper serves merely as an introduction to Dr. Slosson's.

(4) When the matter of the sun has all been stoked into his furnaces and they are gone altogether out another sun will probably have been formed, so that on this earth or on some other earth—it matters not which some billion of years hence—the development of man may still be going on.

ROBERT A. MILLIKAN
NORMAN BRIDGE LABORATORY OF PHYSICS,
CALIFORNIA INSTITUTE, PASADENA
September, 1928

[3] O. Stern, Zeit. f. Elektrochemie, *31,* 448, 1925.

[4] Richard C. Tolman, Proc. Nat. Acad. Sci., *12,* 67, 1926; *14,* 268, 348, 353, 1928.

[5] F. Zwicky, Proc. Nat. Acad. Sci., *14,* 592–597, 1928.

THE RELATION OF PHYSIOLOGY TO OTHER SCIENCES—II

SCIENTIFIC ASPECTS

PHYSIOLOGY takes its place as a science in proportion as its data are accurate and its principles fall into line with those in the other sciences. My great teacher Starling said that science has only one language, that of quantity, and but one argument, that of experiment. The qualitative observations of one generation tend to become quantitative at a later stage of development of a science, and the degree of development of a science can indeed to some extent be judged by the extent to which it falls into a scheme of the

unity of science by giving results which are capable of mathematical treatment and of expression in broad general principles.

I recollect that when I first took up the study of chemistry the acquaintance of most chemists with any of the branches of mathematics was so slight that there was on the market a book on arithmetical chemistry. Shortly after that time the progress of physical chemistry on the Continent had become so definite that it came to be considered quite a useful thing for a chemist to acquire some knowledge of the higher mathematics, and the appearance in Britain of a textbook of higher mathematics for students of chemistry and physics rendered great service by introducing the kind of mathematics that was likely to be of value in application to these subjects.

What has happened in physics and chemistry may be reasonably expected to happen in biology so soon as it is able by improvement in the accuracy of its methods, and by progress in the formulation of its problems, to employ mathematics with profit in the manipulation of data and in the construction of those generalizations which are landmarks of progress in all the sciences; indeed we are, I think, now witnessing the commencement of such a phase in the development of our own subject. The many facets of physiological inquiry make it incumbent on all of us to possess some knowledge of one or more related subjects, and I know of no more promising collateral subject which a young physiologist could take up at the present time, as an alternative to chemistry or biology, than the study of mathematics. But those who do take it up should do so for the purposes of utilizing it in their own experimental work, not merely for the purpose of surveying results obtained by others, and still less in order to "lend an air of verisimilitude to an otherwise bald and unconvincing narrative." Mathematics is a most valuable aid to reasoning, and it can be of no real use to physiology except when it leads to clarification of thought both of an author and of his readers. Under any other circumstances its introduction into biological literature is, I think, of extreme danger, because of the superstition, common alike to those who write and those who read, that anything expressed in mathematical form must be accepted as correct without any further question.

Mathematics and mathematical physics have been of considerable use to physiology in increasing the accuracy of its experimental data, and this in two ways. First, by bringing the accurate experimental and intellectual methods of physics to bear on the construction and use of the numerous physical instruments which it employs. It has been said by Professor A. V. Hill that many of the early investigations on muscle were in reality studies of the properties of levers, and it is certain that similar remarks apply to only too many investigations in which the properties of the apparatus used have not been suitably investigated. As illustrations of the value of mathematical-physical study of apparatus one may mention the classical investigations of Frank on hæmadynamical recording apparatus, the fundamental treatment of string galvanometers and similar instruments by Einthoven, the correction of capillary electrometer records by Keith Lucas, and the vast improvements in galvanometer systems effected by Downing and Hill.

Even when the apparatus at the disposal of the physiologist is unexceptionable, however, it is often the fact that, owing to the nature of the subject, results are not susceptible of repetition with the same ease and certainty as are those of chemical or physical experiments. The variability of the results is due in such cases to what are called accidental circumstances, a term which in reality means circumstances over which we have no control, owing either to our ignorance of their nature, or else to our inability to alter them. In those cases where further study provides methods of more fully understanding and therefore more adequately controlling these circumstances, valuable results follow almost at once. For instance, certain of the obscure causes of different behavior under particular conditions are inborn, and can be controlled by the use of inbred strains of animals such as those of the standard inbred white rats; or, again, one may mention the far-reaching results of the observation by Pavlov that the utmost care must be exercised when studying the conditioned reflexes to exclude all stimuli however trivial they may appear except the one under consideration.

Under the most favorable conditions, however, it has up to the present been usual to find a considerable unavoidable margin of variation in the results of many physiological experiments. By regarding these provisionally as "chance" variations, considerable help may be obtained by the application of the theory of errors, based on the theory of probability. In reality this is an empirical method of which Poincaré has said that "everybody firmly believes in it, because mathematicians imagine that it is a fact of observation, and observers that it is a theorem of mathematics," but nevertheless, although it can not, as seems sometimes to be assumed, be used to replace accurate observation, it does enable a result to be brought out which might otherwise be obscured by small variations beyond our control. Research by such statistical methods provides a useful method of investigation, as, for instance, in the study of the toxic or other action of drugs, the data of the œstrus cycle, etc. An elementary deduction which can be drawn from the con-

sideration of these facts is that, where only a few experiments of any kind are performed, important conclusions can not be drawn unless it can be shown that the conditions are so controlled, and the accuracy of the actual observations so high that the sum of the individual "chance" variations must be small. Observation of this precaution would, in my opinion, reduce the bulk of contemporary physiological literature very materially, with a corresponding improvement in its quality.

Lastly, as a means for evolving generalizations out of experimental data, and of bringing these into relation with the generalizations of other branches of science, the use of mathematics is incontestable. One need only mention as examples the fresh outlook which has been provided for further investigation by the exact study of the data relative to the segregation and recombination of hereditary factors, the beautiful investigations of L. J. Henderson on the equilibria in the blood, the theoretical study of the phenomena of excitation, the employment of thermodynamics and the numerous other applications of physicochemical theory.

Certain applications of physics to physiology are quite clear-cut and need no further comment; but in many respects conventional physics has for our purposes serious limitations, which the physiologist must try to make good by his own investigations. For instance, many hydrodynamical problems of a specialized kind are connected with the study of the circulation. The physical theory of the flow of homogeneous liquids in wide, rigid, unbranched tubes is fairly well established, though, I understand, somewhat abstruse. But when we come to study the physical aspects of a pulsatile flow of a heterogeneous mixture like blood along tubes which are branched and of varying degrees of elasticity, of diameters which in the same system range from several centimeters down to a few microns, and these subject to variations, we can expect little help from orthodox physics, which is not in the habit of working with so many independent variables.

It follows that much of our physics, if it is worth calling that, must of necessity be empirical for the present. This is not a defect in physiology—it is a defect in physical knowledge.

Chemistry and physiology having both originally sprung from the art and practice of medicine, it is little matter for surprise that such a rich harvest has been reaped by their reunion in the form of biochemistry. Although these developments were foreshadowed by the intuition, if not by the actual achievements, of the iatro-chemists of the sixteenth century, little advance was possible until chemistry had, by separation from medicine, established its position as an independent science. So that it was not until about 1840 that organic chemistry and biochemistry were able, chiefly owing to the inspiration of Liebig, to make rapid progress, at least on the Continent. There is probably no branch of chemistry that is entirely without interest to physiology, but of course preference must always be given to organic and physical chemistry. It is significant that at the present time a steadily increasing number of young highly trained organic chemists consider it worth their while to turn to biochemistry; their welcome entry into our ranks gives us fresh hope and faith in our future, as well as in theirs. Already one can point to many achievements of the organic chemist applying himself to our problems, the work of Fischer on the carbohydrates, purine bodies and proteins and amino acids, the more recent work on adrenaline, the identification of carnosine, glutathione, the structure of thyroxin and the natural bases, of which histamine threatens to rival or even to eclipse lactic acid in its importance to the physiologist. As is usually the case, rapid developments in biochemistry have followed improvements of technique; the advances in micro-methods of analysis, without which insulin would probably not have been discovered, or the constitution of thyroxin made known, have played a very important part; the same applies to the whole subject of physical chemistry, much of which, like colloid chemistry and the theories of buffer action, has been built up in response to biochemical requirements. Since the central problems of biochemistry are dynamical, most of its subject-matter must be treated from that standpoint, and here again the debt to physical chemistry must be recognized, particularly in regard to the study of enzyme action, and more recently of interfacial and membrane equilibria, of the molecular structure of surfaces, and of the phenomena of activation and the thermodynamics of oxidation-reduction phenomena.

Whether a biochemist should be primarily a chemist or a biologist is a question which has been much debated in private, though little in public. Personally I see no reason why he should not be both. If he must have one label, it is better that of the chemist, provided always that the biochemist works in the closest possible association with the physiologist. This is most essential if both are not to be deprived of much valuable interchange of ideas and, on a lower plane, of materials and apparatus. In fact, I am convinced that within the limits of administrative possibility the greater the variety of workers brought together the better the results.

So much for the exact sciences. Their value to physiology is immense. They help us to interpret phenomena, but not to predict. In a word physiology is something more than biochemistry and bio-

hysics; it is, and will always remain, a biological ubject.

As its nearest neighbor among the biological iences, zoology should have the closest relations ith physiology, yet it is curious that during several ecades, for reasons which need not now be discussed, hose two subjects were as the poles apart. The ewly distinterred subject of comparative physiology, owever, bears witness to returning interest of oologists in the experimental study of function as gainst mere morphological classification, as well as f physiologists in comparative function as a valuble means of throwing light on their own special problems. For there can surely be no more fruitful means of studying that response to altered conditions which we know as structural adaptation, and which we consider as only a special case of response to a stimulus, than the study by physiological methods of those examples of homology and analogy with which zoological science can so abundantly supply us.

With the science of botany, except in its most general principles, physiology has a less direct connection, though here too the demonstration of fundamental points of resemblance in the metabolism of plants and animals, and the fact of the mutual dependence of the animal and vegetable kingdoms on each other, reminds us that we can not afford to ignore the physiology of any living thing. Nor, in this connection, should we forget that many valuable suggestions have arisen from plant physiology—the discovery of the cell, of Brownian movement, of osmotic pressure, and the notion of the storage of food materials, for instance.

The relation of anatomy to physiology can best be understood if we recall the fact that when the time was ripe physiology separated off from anatomy, taking with it all those dynamic problems which concerned function, and leaving anatomy literally little but the dry bones. The stationary condition of anatomy during the last decades of the nineteenth century was similar to that of zoology, and indeed had similar causes, and was little relieved by the subsequent incorporation of anthropology and embryology. Histology had in most countries remained with anatomy, and had for the most part been content, like it, merely to describe the structure of preserved dead things. In Britain, it is true, histology had until quite recently everywhere remained with physiology, and had perhaps fared no better, for although the British, like their continental friends, did "nothing in particular," they did not do it very well, for we must admit that histology had degenerated into a merely descriptive subject, supplemented by training in a useful technique, and by the identification of specimens. Nevertheless, there were rays of hope, and occasional hints, as in Bowman's researches on the kidney, Hardy's study of the structure of protoplasm, Langley's investigation of the changes in glands during secretion, or more recently Herring's careful study of the pituitary body, that the problems of function had not been entirely lost sight of, and that the large mass of histological information which had been collected might become valuable if only the fundamental question as to the reality of the structures described could be settled.

At the present time some English schools have followed the American and Continental practice, and handed histology over to anatomy, and though I am personally not at all convinced of the justification of this step, yet in view of the indications of quickening in the subject of anatomy during the past two decades, it no doubt is best to suspend judgment as to the ultimate result of the transfer. The portents of the approach of a more live and scientific type of anatomy, of an anatomy of a kind far more useful to physiology and to medicine, are many. The study of the relations of organs in the living body, of the functional significance of structure, the newer experimental histology, as typified by studies with ultra-violet illumination, ultra-microscopy, micro-dissection of live cells, tissue culture, micro-chemistry and the remarkable development of experimental embryology bring to the physiologist joy and hope, and the conviction that the artificial line of demarcation between anatomy and physiology will happily soon be a thing of the past.

The relations of anatomy and physiology to pathology are, or should be, as close as those with each other. When the separation of physiology from anatomy took place many methods and problems which rightly belonged to pathology went with it—such problems of nutrition as inanition, rickets, diabetes, ketosis and acidosis, or jaundice, and of the circulation as heart-block, fibrillation, and so forth. These and many other problems were studied in the physiological laboratory by methods which physiology had come jealously to claim as its own; the dead study of anatomy led to a pathology of the dead in preference to that of the living, and the euphemism so common in the wards, "when this case comes to the pathologist," meaning "when this patient is dead," is significant of this state of affairs. Yet it must be quite apparent that pathology and medical science can only take as their starting-point the study of the normal individual as presented by physiology.

Instead of this, the experimental side of pathology has up to the present been almost entirely directed to the study of bacteriology, which, though well enough in its way, is too narrow and superficial, because it gives insufficient information as to the relation

between bacteria, their products and the tissue cells on which either infection or immunity can be explained. Now that the subject of physiology is so far advanced, the time is ripe, if not overdue, I think, for the pathologist to come into his own, and for the subject of experimental pathology, with ramifications similar to those of physiology, to attract some of the best brains in the world of biological workers. And, if the knowledge of service rendered to their fellows be regarded as payment, they will be well paid.

The subject of psychology was until recently included at the British Association as a sub-section of physiology. As a science psychology must always retain the closest links with physiology, and I think that in the future these links will be strengthened rather than weakened. The researches of Pavlov on the conditioned reflexes will undoubtedly revolutionize the study of physiological psychology, and I need offer no further comment on their scientific excellence, or on the general approval they have won, beyond reminding you that they have already been condemned by Mr. Bernard Shaw.

I have, I hope, said enough to lend emphasis to my principal point, which is that the subject of physiology has the most intimate and vital contact with all biological subjects, with the fundamental sciences, and with medicine. It is, in fact, one of the best possible illustrations of Herbert Spencer's idea that "the sciences are arts to one another." It has often been said that science knows no frontiers and no nationalities. If we apply this a little nearer home we shall all look forward to the day when departments will merely indicate administrative boundaries and not intellectual compartments. In the meantime it is to be hoped that increasing numbers of young people specially trained in other sciences will think it worth while to try to understand what physiology is and what it is striving for, and that they will come to our aid with their own special implements and standpoints.

PHILOSOPHICAL POSITION

Although the application of those sciences which are called "exact" is of immense value to physiology, we must be under no misapprehension as to their real relation, which is merely that they enable the phenomena of life to be described more accurately. They in no way furnish an explanation of those phenomena or enable us, without direct reference to physiological facts, to forecast them. The so-called exact sciences appear to be so because of the simplifications of which they are capable, by reason of which problems can readily be formulated and attacked. Disturbing conditions can provisionally be ignored or allowed for, and a first approximation reached which can be corrected later. In biology this can less readily be done. It is the failure to appreciate this elementary fact which leads some of those trained only in the methods of the exact sciences into the most palpable and unpardonable blunders when they attack biological problems. To take a simple illustration, no amount of pure physics, chemistry and mathematics would have enabled the intricate and beautiful physico-chemical adaptations which have been shown by L. J. Henderson to happen in blood, to have been predicted, because these adaptations depend, among other things, on the presence of membranes round the red cells, fashioned by the living cells and having properties incapable of prediction. The investigation of the equilibria themselves, in their physiological significance, was a necessary preliminary to the introduction of physico-chemical theory. When these phenomena, and deductions from them, became known, it was possible for the physical chemist to step in, apply the appropriate theories, and thus enable the phenomena to be more accurately described in his own language.

But the fact remains that this description turns entirely on the postulated physico-chemical properties of the membranes as deduced from their actual behavior under given conditions in what are in reality physiological experiments. It brings us no nearer to an explanation, perhaps, but it certainly does enable us to link up some of the phenomena of life with phenomena in the non-living, and so to describe them in terms which we think we understand better, because for some reason we regard physics and chemistry as more fundamental sciences than biology. Whether they are really more exact, however, is a point which might be debated.

The process of application of the exact sciences to physiology consists in reality of studying the phenomena themselves and then adopting the most plausible explanation capable of formulation in terms of the exact science. There is no other way. But let us be under no illusion about finding final explanations of what life is by this or any other methods.

The enormously rapid developments of physics in recent years strike the uninitiated onlooker dumb with an almost religious awe. Matter and energy are as fleeting as time, and the ingenuity of man has spanned the mighty extent of the known universe. Matter, energy, time and space are in the melting-pot, and out of it will come we know not what of strange relations of one to another. Of one thing we may be sure—that no final explanation will follow. Lines of separation previously held to be rigid will probably fade away, and there will be found to be a continuity between matter and energy, between living and non-living, between the conscious and the un-

conscious. But since philosophy can not arrive at an explanation of the nature of human understanding, the great mystery of the origin, nature and purpose of life will, I think, always remain to tease, stimulate or humiliate us.

Each must decide for himself what view he takes, and as many of our religious and philosophical beliefs are no doubt unconscious wish-fulfilments, I feel that it ultimately amounts to our decisions being dependent upon our individual temperaments, or, in other words, on our personal physiological make-up.

It was pointed out long ago by Claude Bernard that all *a priori* definitions of life, like those of time, space or matter, are futile, since they usually themselves imply the thing defined. Let us take one or two famous definitions of life as examples. Bichat in 1818 defined life as "the sum total of those functions which resist death." Here we have two opposed ideas, life and death. "All that lives will die; all that is dead has lived." For Bichat life is a struggle of the living thing against an environment which seeks to destroy it, but it is clear that the idea of life as opposed to death is implicit in the definition. This idea of an internal teleological principle, of entelechy, runs through all biological writings back to Aristotle, with whom we believe it to have originated. The amoeba which encysts itself does so in order to defy adverse conditions in its environment. The "calculating intelligence" postulated by Kant directs this response.

Another definition of life which has been much favored of late is the mechanistic one in various forms; "life is a special activity of organized things." Here again the definition implies the idea itself. The possession and maintenance of a definite structure can not any longer be held to be an outstanding feature of living matter as commonly understood, for recent researches in physics show us that, although electrons may come and go, the atomic structure of matter is relatively stable, even though under particular circumstances mutations may occur. Nevertheless the view of life as a mechanism created by and entirely dependent upon its environment gained strength owing to the developments in other sciences, particularly by reason of the synthesis of organic compounds, the principle of the conservation of energy and the introduction of the Darwinian theory of evolution. According to this view, a revival of that of Empedocles, teleological manifestations are accidental. As that thoughtful writer Hjort remarks, however: "When we, as human beings, call a thing accidental, it only means that we give up the hope of understanding it. . . ." "In the physical sciences those factors are termed accidental which we voluntarily disregard in the course of an investigation, or which we find we have omitted to notice." Kant, however, in his "Kritik of Judgment" calls the teleological "the link whereby our understanding can alone be supposed to find any agreement between the laws of nature and our own power of judgment."

Mechanistic interpretations tend in the long run to become arrogant and superficial, as vitalistic ones predispose to scientific nihilism. For, while it is inconceivable that living things do not obey the laws of nature, yet it is equally unthinkable that a chance encounter of physico-chemical phenomena can be the explanation of their existence. This being so, how can we, in Kant's words, "arrive at an understanding of nature"?

It seems clearly impossible to harmonize or to decide between these opposed views of the nature of life, and I do not think any final conclusion to be possible or even necessary. To quote Hjort once more, "Philosophy has no other starting point than a problem, and the current results of scientific research; it never leads to any absolute conclusion. It grows with the science of nature, since in reality it comprises the most general results of that science and comprises nothing more. It does not explain the nature of the human understanding, and provides no means of getting behind the understanding itself . . . the existence of which is the first and necessary condition for the existence of science at all."

Physiologists, in attempting to know what life is, have in my opinion attempted too much, and I think that a new standpoint is essential. One of the greatest of contemporary thinkers, L. J. Henderson, has recently submitted an argument with which I venture humbly to agree. The idea of adaptation, urged by Claude Bernard, should be adopted by physiology as its basal principle, as the chemist accepts the conservation of matter or the physicist the conservation of energy. We need not seek to know why it is so: that is the province of the philosopher; all our experience tells us that it is so. It is not a definition of what life is, but a brief statement of its way, which is valuable, stimulating and true. But we must treat the organism and its environment as one if we are to gain a proper insight into the adaptations manifested by the former. Life is conserved by adaptation, and I venture to think that this conception will be useful alike to general biology, to physiology and perhaps most of all to pathology. For there is no fact in biology, pathology or therapeutics which may not profitably be viewed from this fundamental physiological standpoint. An essentially similar standpoint has been reached by Haldane, who says: "We can reach no other conclusion than that it is the very conceptions of matter and energy, of physical and chemical structure and its changes, that are at

fault, and that we are in the presence of phenomena where these conceptions, so successfully applied in our interpretation of the organic world, fail us." It is the concern of physiology to study the normal functions, and here the normal must be regarded as a statistical group. For particular purposes it is convenient to consider normals as of fixed value; thus the normal man has a body temperature of 37.5° C., a pulse rate of 70, a systolic arterial pressure of 120 mm. Hg, a red cell count of 5,000,000 per cubic mm. or an alveolar carbon dioxide pressure of 40 mm. Hg, etc., and we can investigate the means by which this constancy is reached. But for other purposes it is equally convenient to regard each of these in turn as variable, to study its variations and find how they are produced. When we do so we find with increasing clearness, the more deeply the subject is investigated, that the variability and the constancy are closely related, the fixed value of one thing being due to the interplay of the variables of others. Thus the constancy of the alveolar CO_2 pressure may be regarded as due to the interaction of such variables as hydrogen ion concentration of blood, body temperature, ventilation rate, oxygen pressure, etc., by which a state of equilibrium is maintained.

We have in the study of physiology many beautiful examples of this closely woven texture of interdependent phenomena. Modify any condition concerning any one of them, and you at once set the machinery moving in such a way as to counteract what you have done. And this is not what life is but what it does, which distinguishes it—it adjusts the organism to its environment.

There is a striking though superficial resemblance between this principle of biological adaptation and the principle of Le Chatelier of "the opposition of a reaction to further change" which is expressed "when any system is in a state of physical or chemical equilibrium, a change in one of the factors of equilibrium will cause a reverse change within the system."

In living things, however, as Donnan has remarked, "the activities, and indeed the very existence, of a living organism depend on its continuous utilization of an environment that is not in thermodynamic equilibrium. A living organism is a consumer and transformer of external free energy, and environmental equilibrium means non-activity and eventual death." Nevertheless, as Claude Bernard believed, and as Henderson has strikingly illustrated, the internal environment is maintained very constant in certain respects, and this constancy is the outcome of special activities which characterize life.

Glancing now towards the future, what may we say represents in a few words the trend of modern physiology? In many ways a great future lies before it. Utilizing the other sciences as its tools and itself reacting powerfully on them, we can confidently predict progress to undreamed-of heights, an enormous development of experimental pathology and medicine, and far-reaching effects on economic and sociological conditions. Yet, implicit in these very potentialities, there is another and a gloomier side to the picture. The rapidly accumulating wealth of detailed knowledge and of special technique demands an increased specialization; unless there is a periodic intellectual stocktaking there must inevitably be a loss of perspective and of grasp of great general principles. But how can this stocktaking be done? Can team work ever reach that harmony of action which distinguishes the individual? Any scientific subject is capable of indefinite expansion, and with the biological sciences it is hard to foresee what the ultimate end of *mere* expansion can be. How will scientific literature develop? Will there have to be abstracts of abstract journals and reviews of reviews? Will the subdivision of the subject necessitate in the long run the creation of lectureships or professorships to deal, for example, with the special physical chemistry of heterogeneous equilibria in biological systems, with intermediary metabolism, with the problems of hemodynamics or growth or reproduction? If so, how will the results of their special investigations be brought to common ground if no great unifying principles come to light? Can we expect that such unifying principles will appear: if they do not, will the progress of science be brought to an end by the accumulation of its own products?

The establishment of special research professorships, however profitable in isolated cases, can not in my opinion make good this growing specialization, because it will tend to divorce research and teaching and place the teaching professor on a level of real or apparent inferiority. The idolization of research for the sake of the advancement it brings is another of the dangers which threaten us. If there is one thing worse than "a mediocrity who does no research" it is "a mediocrity who does." There are at the present time a large number of junior research posts available, but not enough well-trained people adequately to fill them. This is all to the good provided that those who on trial show no aptitude for the work can be ruthlessly eliminated. As they often can not, there are in consequence a number of young people who drift from one research scholarship to another, perhaps not aimlessly, but with no better objective than the manufacture of papers designed to justify their employment. The hapless editors of each of the swelling tide of journals are coaxed, hoodwinked and, if necessary, bullied, to ensure that these papers see the light of day. In the fullness of time the list of

short-time research posts is exhausted, and the young investigator must now either turn to some entirely different occupation or else, as one of my friends expressed it, "subside into a professorial chair" for which, incidentally, he is probably entirely unfitted. The pursuit of science is nowadays, perhaps unfortunately, a career, and one in which moreover it pays to advertise. Science, we are often told, is the cream of civilization. If we believe this let us use all our endeavors to ensure that it be not a whipped cream, specious, puffed up with wind, and presenting a fictitious appearance of solidity.

CHARLES ARTHUR LOVATT EVANS

MEMORIAL OF ALPHEUS HYATT

ON the request of the executive committee of the Marine Biological Laboratory, Mrs. Alfred G. (Harriet Hyatt) Mayor, daughter of Alpheus Hyatt, prepared a *bas-relief* and memorial tablet in bronze of her father, which was unveiled in the reading-room of the laboratory on September 4, 1928. In presenting the tablet to the laboratory, on behalf of Mrs. Mayor and her family, Professor E. G. Conklin, of Princeton University, made the following remarks:

Alpheus Hyatt was the leader in the movement which resulted in the establishment of the Marine Biological Laboratory at Woods Hole. It is often said that this laboratory is the lineal descendant of the Anderson School of Natural History, established by Louis Agassiz on the island of Penikese in 1873; but this is true only in the sense that several persons who were associated with that school were instrumental in founding this laboratory.

In 1870, before the establishment of the school at Penikese, Professor Hyatt had organized a Teacher's School of Science which continued under his guidance for more than thirty years. Lectures and laboratory work for teachers were given by him and by several other distinguished scientists. "Science Guides," which were the precursors of our "Laboratory Directions," were prepared by him and his associates for this work, and more than twelve hundred teachers received instruction. In furtherance of this work, Professor Hyatt, with the aid of the Woman's Education Association of Boston and the Boston Society of Natural History, maintained a sea-side laboratory at Annisquam, Massachusetts, from 1880 to 1886. After the session of 1886, Professor Hyatt called a meeting at the Boston Society of Natural History of those residents of Boston interested in the founding of a more permanent and better equipped laboratory, and in March, 1888, the Marine Biological Laboratory was incorporated by ten residents of Boston, the first name on the list being that of Alpheus Hyatt. Professor Hyatt was for two years president of the corporation and was a leading member of the board of trustees. It is most appropriate, therefore, that we should commemorate in beautiful and enduring bronze our debt of gratitude to the man who more than any other one person was the founder of this laboratory.

Since a generation has arisen that knew him not, it is well on this occasion to recall some of the salient features of his life and work.[1] Born in 1838 at Washington, D. C., he died suddenly at Cambridge, Massachusetts, in 1902. He was a student of Louis Agassiz at Harvard and graduated from the Lawrence Scientific School in 1862. Among his fellow students were Alexander Agassiz, Scudder, Putnam, Shaler, Verrill, Morse and Packard. After graduation he served in the Union Army throughout the Civil War and was retired with the rank of Captain.

In 1867 he was associated with E. S. Morse, A. S. Packard and F. W. Putnam in the Peabody Institute at Salem. In 1870 he was appointed custodian of the Boston Society of Natural History and in 1881 he became curator and continued in that office until his death. From 1870 to 1888 he was professor of zoology and paleontology in the Massachusetts Institute of Technology, and from 1877 until his death he was professor of biology in Boston University. He was one of the founders of the American Society of Naturalists, a member of the National Academy of Sciences, and an honorary member of many foreign scientific societies.

His scientific work was both extensive and intensive; he was a stimulating teacher of zoology and paleontology, a distinguished museum administrator, an organizer of scientific societies, schools and laboratories; but in addition to all these he was an important contributor to knowledge. His greatest works were on fossil cephalopods, culminating in his monumental monograph, "Genesis of the Arietidae" (Smithsonian Contributions to Knowledge, 1889); but he also made important contributions to our knowledge of sponges, bryozoa, pelecypoda, gasteropoda and insecta. Almost all these studies have to do with the evolution and genetic relationships of these groups of animals. He said of himself that he had been an evolutionist since 1859, the year of the publication of Darwin's "Origin of Species," and, incidentally, the year in which he became a student under Agassiz. His most important contributions to evolution consisted in detailed comparisons of the stages of ontogeny with those of phylogeny, for which study the fossil cephalopods were peculiarly favorable since the stages of the individual life history as well as the geological succession of species were represented in the characters of the skeleton. He divided the whole course of ontogeny into ten principal stages and he pointed out the resemblances between these stages in the life history of the individual and the life history of a species. Among the many generalizations which he developed from these studies, perhaps the best known is his "law of embryonic acceleration," ac-

[1] Much of what follows has been drawn from the "Memorial of Professor Alpheus Hyatt" published in the *Proceedings* of the Boston Society of Natural History, 30: No. 4, June, 1902; and from Dr. Robert Tracy Jackson's paper entitled, "Alpheus Hyatt and his Principles of Research," *The American Naturalist*, 47: April, 1913.

cording to which "features appearing at or near the adult period are inherited at earlier and earlier stages in successive generations."

Professor Hyatt's personal characteristics endeared him to all who knew him. He was courteous, unselfish, sincere, free from jealousy and envy; he "could not abide shams, either scientific or social," and yet he was tolerant, genial and kind. One who knew him well called him "a noble man, a faithful friend, a great scientist." His widow still lives at an advanced age and two daughters survive, both of them distinguished artists, to one of whom we owe this beautiful tablet which we formally unveil to-day. May it serve to remind many future generations of workers in this laboratory of their debt to a great and good man who sowed that others might reap—who labored that others might enter into his labors.

Professor Frank R. Lillie, president of the corporation, in accepting the tablet, said: "As a successor of Professor Hyatt in the office of president of the corporation of the Marine Biological Laboratory, I hereby accept this memorial in the name and on behalf of the laboratory. I express to the artist and donor, Professor Hyatt's daughter, our appreciation of her gift for its beauty and its significance, and pledge enduring memory of him who transmitted the influence of Louis Agassiz from Penikese to this place."

In addition to a striking portrait of Professor Hyatt, the tablet bears the following inscription:

ALPHEUS HYATT

First President of the Woods Hole Laboratory 1888. He also founded its prototype at Annisquam, Massachusetts, established in 1880 with the aid of the Woman's Education Association and the Boston Society of Natural History.

1838–1902.

SCIENTIFIC EVENTS

THE BRITISH ASSOCIATION

As already announced, the meeting of the British Association for the Advancement of Science for 1929 is to take place in South Africa, under the presidency of Sir Thomas Holland, rector of the Imperial College, South Kensington. We learn from the London *Times* that it is to be a week earlier than had been expected, to suit the convenience of the hosts. It will begin in Cape Town on July 22, 1929; there will be a brief visit to Kimberley, July 29–30; the presidential address will be delivered at Johannesburg on July 31. As there are geological and agricultural congresses at Pretoria from July 31 to August 7, various sections of the British Association will meet at Pretoria in connection with these.

An invitation from the French Association and the city of Havre, presented by Dr. Adrien Loir, curator of the Havre Natural History Museum, was cordially accepted, that the members unable to go to South Africa should join the meeting of the French Association at the end of July next year. An exactly similar invitation was received in 1914 when the British Association met in Australia, but the war made it necessary to cancel the Havre meeting.

At the Glasgow meeting a deputation from Bristol, headed by the Lord Mayor and a representative of the vice-chancellor of the university, invited the association to meet in Bristol in 1930, and this was cordially accepted. In 1931 the centenary of the association is to be celebrated, and an invitation was offered from York, where the first meeting was held. In view, however, of the expectation that that meeting will be unusually large, there is considerable doubt if accommodation could be found in York. After discussion it was agreed that it would be suitable and appropriate to hold the centenary meeting in London, the center of the Empire, and the council was authorized to accept any invitation they might receive. A deputation consisting of the Lord Mayor and a representative of University College, Leicester, invited the association to that city, and it was agreed to accept, either for 1932 or 1933, leaving it open in the meantime to begin the second century of the association, like the first, at York.

Since last year the association has been granted a Royal Charter of Incorporation. The statutes appended as a schedule to the charter had already been approved by a special meeting of the general committee held in London early this year. Draft regulations supplementary to the statutes were submitted at Glasgow and formally passed. The thanks of the association were given to Mr. A. A. Campbell Swinton, F.R.S., who had generously defrayed the cost of the charter and the expenses incidental to its acquisition. As the association is now able to hold funds and property in its own name, its securities, hitherto held by Major P. A. MacMahon, Sir Arthur Evans and the Honorable Sir Charles Parsons, have been transferred to it, and the trustees were thanked for their services.

THE WORLD AGRICULTURAL CENSUS

ARRANGEMENTS for taking a world agricultural census in 1930 have been practically completed, the countries, colonies and mandate territories which have promised active participation in the census representing approximately 98 per cent. of the agriculture of the world, according to Leon M. Estabrook, director of the census for the International Institute of Agri-

culture at Rome. Mr. Estabrook was formerly chairman of the crop reporting board of the Department of Agriculture, and was loaned by the Department to the International Institute. He is at present making a brief visit to the United States.

The results of the census will be published probably in 1931 or 1932. The figures will give the number and size of farms, the area and production of important crops, and the number of each kind of livestock by age and sex classifications. In the Northern Hemisphere the census will be taken after the crops of 1929 are harvested, probably in the early months of 1930. In the Southern Hemisphere, where seasons are reversed, the census will cover crop production of the year beginning July 1, 1929, and ending June 30, 1930, probably in the early months of the winter there.

Each country participating in the work will take the census with its own organization and methods, following a standard form furnished by the Institute of Agriculture, and will summarize and publish its own results. These results when available will be brought together in world tables and published by the institute at Rome, probably in 1931 or 1932.

The preliminary work of organization of such a census was made possible by an allotment of funds from the International Education Board; a small allotment of funds, office space and clerical service by the institute at Rome, and the loan of a specialist by the Department of Agriculture with a contribution to his expenses.

Mr. Estabrook in a recent statement reports that of the 200 countries listed by the institute, only 60 have ever taken an agricultural census, and of these less than 40 have taken a census since 1900; of these 40 not more than three or four by mere chance happened to take their censuses in the same year. The preliminary work of the world agricultural census began in 1925 with a study of all agricultural censuses that have been taken since 1900. A program and relatively simple standard form of census questionnaire was prepared and approved by the General Assembly of the Institute early in 1926, and copies were sent to all governments of the world with a request for their cooperation.

ACTIVITIES OF THE ROCKEFELLER FOUNDATION

DURING 1927 the Rockefeller Foundation, in disbursing from income and capital $11,223,124, (1) aided local health organization in eighty-five counties of six states in the Mississippi flood area; (2) operated an emergency field training station for health workers in this region besides contributing toward the support of nine other training centers elsewhere; (3) assisted nine schools or institutes of public health and three departments of hygiene in university medical schools; (4) gave aid to seventeen nurse training schools in nine countries; (5) furnished funds for land, buildings, operation or endowment to nineteen medical schools in fourteen countries; (6) supported the Peking Union Medical College; (7) paid two million dollars toward a new site for the University of London; (8) helped Brazil to maintain precautionary measures against yellow fever; (9) continued studies of that disease in West Africa on the Gold Coast and in Nigeria; (10) had a part in malaria control demonstrations or surveys in eight states of the southern United States and in eleven foreign countries; (11) aided nineteen governments to bring hookworm disease under control; (12) contributed to the health budgets of 268 counties in twenty-three states of the American Commonwealth and of thirty-one similar governmental divisions in fourteen foreign countries; (13) helped to set up or maintain public health laboratory services or divisions of vital statistics, sanitary engineering or epidemiology in the national health services of nineteen countries abroad and in the state health departments of sixteen American states; (14) made grants for mental hygiene work in the United States and Canada; (15) provided funds for biological research at the Johns Hopkins University and aided investigations in this field at Yale University, the State University of Iowa, the University of Hawaii, the Bernice P. Bishop Museum in Honolulu, and certain universities of Australia; (16) helped the League of Nations to conduct study tours or interchanges for 125 health officers from forty-four countries, to supply world-wide information about communicable diseases, to train government officials in vital statistics and to establish a library of health documents; (17) provided, directly or indirectly, fellowships for 864 men and women from fifty-two different countries, and paid the traveling expenses of 115 officials or professors making study visits either individually or in commissions; (18) made minor appropriations for improving the teaching of the premedical sciences in China and Siam, for the operating expenses of hospitals in China, and for laboratory supplies, equipment and literature for European medical centers which have not yet recovered from the after-effects of the war; (19) lent staff members as consultants and gave small sums for various purposes to many governments and institutions; (20) made surveys of health conditions and of medical and nursing education in fourteen countries.

TRIBUTE TO DR. W. J. HOLLAND

ON the occasion of the eightieth birthday of Dr. W. J. Holland, of Pittsburgh, which occurred on August

16, during his attendance at the sessions of the Fourth International Entomological Congress at Ithaca, N. Y., at a dinner given in his honor in the Willard Straight Hall of Cornell University, Dr. A. Avinoff, the present director of the Carnegie Museum, presented him with an illuminated manuscript on behalf of his friends in the museum.

This manuscript, done in natural colors and gold on parchment, bears, encircled in a laurel wreath, the Roman numerals LXXX. Within the reflected radiance of these golden numerals and around the initial letter of the text are depicted a swarm of various species of brilliantly colored butterflies, while at the base of the manuscript is a great Dinosaur, all of which have been made known to science by Dr. Holland. Entwined or resting upon a symmetrical conventionalized floral scroll, adorning the border in the style of ancient illuminated manuscripts, are numerous examples of plants, both recent and fossil, insects, fishes, reptiles, a humming-bird and a fossil mammal, all of which at one time or another have been named in honor of Dr. Holland by various specialists.

Serving as a border to the script and alternating with reproductions of the numerous foreign decorations which have been bestowed upon Dr. Holland in recognition of his services to science, is a series of diplomas, each bearing the appropriate university colors, representing the academic honors which have been conferred upon him, including the degrees of Ph.D., D.D., LL.D., Sc.D. and L.H.D.

A series of volumes bearing the titles Entomologia, Paleontologia, Philosophia and Biologia, emblematic of the principal fields of Dr. Holland's scientific labors, complete the decoration.

This manuscript, of which a reproduction will shortly appear in the "Annals of the Carnegie Museum," is the creation of Dr. A. Avinoff, director of the Carnegie Museum. The Gothic script, in black with red initials, was executed by Mr. Rudyerd Boulton, also of the museum staff.

The text of the document follows:

Dr. Holland:

The Carnegie Museum, created by you and brought largely by your efforts to its present high standard of scientific merit and educational efficiency, salutes you cordially and devotedly on your eightieth birthday. We are proud of your achievements and leadership in so many fields during these years of fruitful and diligent work. As minister of the Gospel you have always closely interwoven your life with that of the church, and you have consecrated yourself whole-heartedly to the study of the wondrous realm of nature. Your place in the forefront of science is worthily and universally recognized. You are eminently identified with the cause of education. You have contributed generously and enduringly to the progress of our community. Your attainments are blossoming richly in the ripe age of your life when the anniversary of your fourscore years finds you as active and vigorous as ever, engaged in the numerous absorbing tasks which are the joy of your life and the inspiration of your friends, admirers and collaborators. As a widely renowned scientist and educator, as an accomplished man of letters, as a prominent churchman and citizen, you have been honored with many just tributes on the part of civic bodies and seats of higher learning in this country and in foreign lands. In trusting that you may be spared for many, many, useful years to come, we shall continue as ever to hold your name, not only high in our unanimous and profound esteem, but deep and secure in our hearts.

Long live our respected and beloved dean!

CARNEGIE MUSEUM,
Staff of the Museum
Museum Committee of Trustees

SCIENTIFIC NOTES AND NEWS

THE autumn session of the National Academy of Sciences will be held at Union College, Schenectady, New York, on November 19, 20 and 21.

DR. FRANZ BOAS, professor of anthropology at Columbia University, was elected president of the International Congress of Americanists at its recent meeting in New York City.

ON the occasion of the meeting of the British Association for the Advancement of Science, honorary degrees were conferred by the University of Glasgow as follows: The Reverend Professor Shailer Mathews, dean of the Divinity School of the University of Chicago, received the doctorate of divinity. The doctorate of laws was conferred on Sir William Bragg, president of the association; Sir Thomas Holland, president-elect; Dr. Adrien Loir, curator of the Natural History Museum at Havre, representing the French Association for the Advancement of Science; Dr. Frank Lincoln Stevens, professor of plant pathology in the University of Illinois, representing the American Association for the Advancement of Science; Dr. Franz Eduard Suess, professor of geology at Vienna; Dr. E. Westermarck, professor of sociology in the University of London, and Professor P. Zeeman, the physicist of the University of Amsterdam.

AT the July congregation of degrees of the University of Wales, the degree of doctor of science was conferred upon Professor Walter Thomas, of the Pennsylvania Agricultural Experiment Station.

DR. S. J. HOLMES, professor of zoology at the University of California, has been elected president of

the California chapter of the Phi Beta Kappa honorary society.

PROFESSOR ALBERT A. MICHELSON, having completed his experiments on the speed of light at Pasadena, California, has returned to Chicago, and expects to resume his lectures at the university on October 1.

THE daily papers report that Dr. George W. Crile, director of the Cleveland Clinic and Hospital, is recovering from injuries received through a fall from his horse.

COLONEL HARRY BURGESS, of the engineering corps of the U. S. Army, has been appointed governor of the Canal Zone.

PROFESSOR GODFREY HAROLD HARDY, Savillian professor of geometry at the University of Oxford, is taking up his work in Princeton University. He is exchanging with Professor Oswald Veblen, of the department of mathematics. Dr. Hermann Weyl, professor of higher mathematics in the University of Zurich, will occupy the Thomas D. Jones research professorship of mathematical physics.

WILBUR A. NELSON has resigned as state geologist of Virginia. For the past three years he has held the joint position of state geologist and head of the school of geology at the University of Virginia. Beginning on September 1, these two positions were separated and Mr. Nelson will devote his entire time to his work at the University of Virginia. Mr. Linwood H. Warwick, who has been chief clerk of the survey since its organization in 1908, has been appointed acting head of the survey, the offices of which will be at the University of Virginia as heretofore.

WM. D. JOHNSTON, JR., has been appointed assistant geologist and Ralph M. Leggette junior geologist in the water resources branch of the U. S. Geological Survey. Francis G. Wells has been appointed junior engineer in the same branch of the survey.

THE committee on government problems of the National Academy of Sciences, consisting of the president and vice-president of the academy, the chairman of the National Research Council, the chairman of the division of federal relations of the council, the chairman of each of the sections of the academy, and one member at large, is now as follows: John C. Merriam, chairman (member at large); T. H. Morgan, president of the academy; F. E. Wright, vice-president; George K. Burgess, chairman of the National Research Council; George Otis Smith, chairman of the division of federal relations of the council, and the following chairmen of sections; W. S. Adams, astronomy; G. D. Birkhoff, mathematics; Marston T. Bogert, chemistry; J. McKeen Cattell, anthropology and psychology; K. T. Compton, physics; W. F. Durand, engineering; R. A. Harper, botany; W. H. Howell, physiology and pathology; H. S. Jennings, zoology and anatomy; G. P. Merrill, geology and paleontology.

DR. WALDEMAR KAEMPFFERT, director of the Rosenwald Industrial Museum, in Chicago, will sail for Europe in October. Mr. Kaempffert will spend about three months in visiting the industrial museums of Munich, Vienna and London.

DR. B. TROUVELOT, of the Ministry of Agriculture at Paris, will spend six weeks at the Citrus Experiment Station of the University of California. He is investigating biological control work. Dr. Efflaton, of Egypt, and W. B. Gurney, entomologist of New South Wales, will also work at the station.

PROFESSOR A. VOLKART has recently returned to Zurich, after visiting Canada and the United States. He visited the leading Canadian experiment stations, traveled down the Pacific Coast to Los Angeles, studied the problems of irrigation and dry-land farming in the southwest and in Kansas, visited the Iowa and Wisconsin Agricultural Experiment Stations and spent a week in Washington, becoming familiar with the work of the Bureau of Plant Industry, U. S. Department of Agriculture. Professor Volkart is gathering material for a revision of "Die besten Futterpflanzen."

WM. B. GURNEY, government entomologist in the Department of Agriculture, Sydney, New South Wales, spent August 23 and 24 inspecting the entomological work of the agricultural experiment station of the Kansas State Agricultural College. Dr. R. T. Hillyard, commonwealth entomologist of Australia, and Mr. F. Muir, entomologist of the experiment station of the Hawaiian Sugar Planters' Association, also visited the department of entomology and made a trip to the insect fossil beds near St. Elmo, Kansas.

DR. J. J. GALLOWAY, of the department of geology of Columbia University, spent the summer in Oklahoma in research on the micropaleontology of the Pennsylvanian formations of the mid-continent region. The work was done in collaboration with Dr. Charles Ryniker, of Tulsa, Oklahoma, and was under the auspices of the Gypsy Oil Company.

DR. MARGARET MEAD, ethnologist in charge of the South Sea Island Hall of the American Museum of Natural History in New York, recently lectured before the faculty and graduate students of the department of anthropology of the University of Cali-

fornia. Dr. Mead is on her way to the Admiralty Islands to make a psychological study of young children.

LORD MELCHETT, formerly Sir Alfred Mond, is visiting the United States. While in America he will lecture at Harvard University on "Industrial Cooperation."

DR. C. M. CHILD, professor of zoology at the University of Chicago, will deliver the first Harvey Society Lecture at the New York Academy of Medicine on October 11. His subject will be "Senescence and Rejuvenescence from a Biological Standpoint."

DR. H. FOSTER BAIN, secretary of the American Institute of Mining and Metallurgical Engineers, was the speaker at the convocation exercises on September 27 of the Case School of Applied Science.

PROFESSOR FERDINAND BLUMENTHAL, director of the Institute for Cancer Research in Berlin, recently lectured in Paris on the present status of the cancer problem.

THE Lane medical lectures of Stanford University will be given this year by Dr. F. d'Herelle, professor of bacteriology at Yale University, during October. The titles and dates of the lectures are as follows: October 22, "Bacteriophagy"; October 23, "Bacterial Mutations"; October 24, "The Nature of the Bacteriophage"; October 25, "Infectious Diseases," and October 26, "The Phenomenon of Recovery."

IN conformance with the request of donors to a scholarship fund in honor of the late Professor E. J. Wickson, formerly dean of the College of Agriculture, the regents of the University of California have changed the name of the fund to the Wickson Memorial Loan Fund, the income from which will be available to needy agricultural students, particularly in the graduate school.

DR. BENJAMIN WARNER SNOW, professor of physics at the University of Wisconsin, died on September 21 at the age of sixty-eight years.

SIR HORACE DARWIN, F.R.S., chairman of the Cambridge Instrument Company, known as an inventor of scientific instruments, died on September 23 at the age of seventy-seven years. Sir Horace was a son of Charles Darwin and brother of the late Sir George Darwin, professor of astronomy and experimental philosophy at the University of Cambridge; of Sir Francis Darwin, professor of botany at Cambridge, and of Major Leonard Darwin, of the Royal Engineers, known for his work in eugenics.

THE nineteenth meeting of the German Society of Naturalists and Physicians was held in Hamburg from September 15 to 22. There were thirty-five sections and almost as many affiliated societies.

THE International Mathematical Congress, which met at Bologna on September 10, held its final session in the Palazzo Vecchio, Florence. The decision to close the congress at Florence was made in homage to the great scientific traditions of the city. During the conference the delegates visited the monuments of Bologna, Ferrara and Ravenna, laying a wreath on the tomb of Dante.

THE American College of Surgeons will hold its eighteenth clinical congress in Boston from October 8 to 12. The address of the retiring president will be given by Dr. George D. Stewart, of New York, and the inaugural address by the newly elected president, Dr. Franklin H. Martin, Chicago. The John B. Murphy oration on surgery will be by Professor Vittorio Putti, of Bologna, Italy, and the fellowship address by Dr. William J. Mayo, of Rochester, Minnesota. Ether Day will be celebrated in the Dome Room of the Massachusetts General Hospital on Friday, when a bronze bust of William T. A. Morton will be presented to the hospital. The chairman of the Boston committee on arrangements is Dr. Frederic J. Cotton. Among the distinguished guests and visitors from abroad will be Dr. Daniel J. Cranwell, Argentina; Sir Charles P. B. Clubbe and Sir George A. Syme, Australia; Sir Charles Ballance and Sir Samuel Squire Sprigge, England; T. de Martel, France; Ulises Valdes, Mexico; Archibald Young, Scotland, and Sir John Lynn-Thomas, Wales.

THE American Society of Mechanical Engineers reports that, since the inauguration of the Engineering Index Service in January of this year, this comprehensive index to all technical periodicals has gained recognition and support from many industries, both domestic and foreign. The old annual index, in volume form, contained from 18,000 to 20,000 items, whereas the new service has annotated nearly 32,000 articles in the first nine months of its existence and the original estimate of 50,000 items per year is, apparently, not amiss. The service in its present form is a complete index to all current technical periodicals from thirty-seven countries. In it one finds a complete résumé of all published technical information with the assurance that further search is unnecessary.

THE one hundredth anniversary of the founding of the College of Pharmacy of the City of New York and the twenty-fifth anniversary of its affiliation with Columbia University will be celebrated in the week of May 27, 1929.

THE Brooklyn College of Pharmacy is to become a part of Long Island University. The consolidation is

to take place before the College of Pharmacy begins the construction of a new $500,000 plant adjoining its present building on Nostrand Avenue, near Lafayette Avenue, which is expected to be fully completed within a year. The college was established in 1887 by the Kings County Pharmaceutical Society as a lecture center, but did not become a technical college until 1891. Approximately 3,000 students have been graduated from the institution and its classes number about 250 every year.

THE American Museum of Natural History is made the remainder beneficiary of trust funds aggregating more than $500,000, under the will of the late Frederick G. Voss. Before his retirement Mr. Voss was U. S. manager of foreign insurance companies. The will directs that the fund be used in further development of the institution's department of research in anthropology and archeology. The museum is the residuary legatee and will receive the principal of trust funds on the death of the life tenants.

THE Academy of Natural Sciences of Philadelphia has contributed $500 toward the completion of the fund needed for the Byrd Antarctic Expedition.

UNDER the auspices of the Industrial Development Board of Manitoba, a council has been formed to direct and stimulate scientific research on problems which have to do with the utilization of the resources of the province. The council has been formed with the assistance of the National Research Council at Ottawa and membership of the board includes engineers, chemists and manufacturers. Provincial industries will be asked to submit their problems and those of national interest will be submitted to the National Research Council at Ottawa.

THE new observatory for studying the northern lights at Tromsö, Norway, was completed and taken into active service this summer, under the direction of the Norwegian Society for Cosmic Physics. Aside from the northern lights, the observatory will also be devoted to the study of atmospheric conditions in high altitudes, and it will conduct magnetic and meteorological observations of great practical value in weather forecasting. The work is in the nature of a continuation and expansion of that of the Geophysical Institute at Bergen, with which it will be closely connected.

A new bee-culture field station, known as the Southern States Bee Culture Field Laboratory, has been established by the Department of Agriculture at Baton Rouge, La. Special funds for this purpose were appropriated by the last Congress. The Louisiana State University is providing laboratory facilities, heat, light and janitor services, and is also assisting financially in the procurement of scientific apparatus. W. J. Nolan, of the department's bee-culture laboratory, Somerset, Md., has been in Baton Rouge since early in July making necessary arrangements for conducting the experimental work. Dr. W. W. Whitcomb, Jr., recently appointed a member of the department's bee-culture staff, is also at Baton Rouge, where he will be stationed permanently.

WITH the approval of the Personnel Classification Board, the Bureau of Plant Industry has recently established the position of principal horticulturist in charge of horticultural crops and diseases. The establishment of this new position is with the intention of accomplishing a regrouping and consolidation in the supervision of several closely related activities. The new principal horticulturist will be assigned to the general supervision not only of the horticultural, pomological and related items of the present Office of Horticulture, but also the physiological project of the Office of Plant Geography and Physiology, the projects relating more or less directly to horticultural crops of the pathological laboratory and of the Office of Vegetable and Forage Diseases, and the entire office of Fruit Diseases and the Office of Crop Physiology and Breeding. It is believed that the more intimate correlation of these related activities will facilitate the cooperative research upon many important complicated problems of production of horticultural crops, not only among the specialists of the new administrative organization but also with specialists of the state agricultural experiment stations and other institutions and with the horticultural industries.

WE learn from *Nature* that Mr. J. G. Pearce, director of the British Cast Iron Research Association, has been unanimously awarded by the judges the first prize of 100 guineas for a series of practicable proposals relating to "Goodwill in Industry." The competition was organized by the Glasgow and West of Scotland Association of Foremen Engineers and Draughtsmen, and the judges represented the three parties in industry—the employer, the worker and the technical or administrative officer.

UNIVERSITY AND EDUCATIONAL NOTES

THE new Hilles Laboratory of Applied Science of Haverford College will be completed in time for the opening of the second semester.

THOROUGHLY equipped biological and chemical laboratories have been installed at Long Island University. The biological laboratory is designed to accom-

modate 192 students and the chemical laboratories 480 students.

AT the University of Virginia, Dr. Sydney William Britton, of the Johns Hopkins University, has been appointed professor of physiology as successor to Dr. Homer W. Smith, who has become head of the physiological department of New York University.

DR. EDWARD F. CASTETTER, associate professor of botany in the Iowa State College, has resigned to accept the headship of the department of biology of the University of New Mexico at Albuquerque.

A. BRAZIER HOWELL, secretary of the American Society of Mammalogists, who for the last six years has been connected with the U. S. Biological Survey and the U. S. National Museum, has been appointed lecturer in comparative anatomy at the Johns Hopkins Medical School. Dr. Clarence E. Ferree has been made resident lecturer in ophthalmology and director of the laboratory for physiological optics. His wife, Dr. Gertrude Rand, will be associate professor of ophthalmology.

PROFESSOR E. A. MILNE, Beyer professor of applied mathematics in the University of Manchester, has been appointed as from January 1, 1929, to the Rouse Ball professorship of mathematics at Oxford. Under the will of Mr. W. W. Rouse Ball, of Trinity College, Cambridge, money was bequeathed for the foundation of Rouse Ball chairs of mathematics at Oxford and Cambridge. Early this year Professor J. E. Littlewood was appointed to the Cambridge chair.

DISCUSSION AND CORRESPONDENCE

SPARING ACTION OF FAT ON THE ANTI-NEURITIC VITAMIN

IF fat be added to an almost fat-free diet,[1] the amount of the anti-neuritic vitamin required to establish any definite level of growth or frequency of ovulation is always less than is required when fat is absent; in other words, fat acts to spare the water soluble anti-neuritic vitamin. Animals on inadequate levels of the vitamin are not only more gravely impaired in stature and ovulation when fat is absent than when present, but they invariably develop fatal beriberi sooner. A level of anti-neuritic vitamin can be found, for instance, on which all animals deprived of fat die in slightly over a month, but on which animals allowed ten per cent. of dietary fat invariably live over four months. Furthermore, fat-free animals near death from beriberi when given only three doses of rice-polish-extract resume more energetic growth if also shifted to a diet with fat present; in spite of this growth and hence increased body substance they also come down later with their second attack of beriberi. Our experiments have been performed with adequate levels of the other water soluble vitamin B (P P) and of the fat soluble vitamins A, D and E, so that we are not at liberty to assign the remarkable favorable action of fat to increased amounts of these substances. Nor have we been able thus far to establish the presence of the anti-neuritic vitamin itself in the fats used. It would appear that we must recognize their mediation in those unknown metabolic processes for which anti-neuritic vitamin is essential. The clear-cut facts herein reported go far toward explaining the beneficial effect of fats[2] recently emphasized by us in earlier studies on highly purified diets.

HERBERT M. EVANS,
SAMUEL LEPKOVSKY
UNIVERSITY OF CALIFORNIA

[1] We have employed Diet 542 (casein—extracted one week with acid water—20, cane-sugar 70, autoclaved yeast 10, salts 4, 2 drops cod-liver oil (Patch) daily) and Diet 550 (casein 20, cane-sugar 59, lard 10, autoclaved yeast 10, salts 4, 2 drops cod-liver oil (Patch) daily). For both diets the anti-neuritic vitamin was furnished at various levels by separately fed unautoclaved yeast. E is administered as a few drops of wheat germ oil daily.

NOTATION IN ATOMIC STRUCTURE

IN most books and papers on atomic structure little attempt is made to distinguish between frequencies and wave numbers except where it becomes necessary to give numerical examples. Both these quantities are usually designated by the letter ν.

It is our practice to use a tilde over the symbol when it indicates wave number, *i.e.*, $\tilde{\nu} = \nu/c$. Similarly, when dealing with the Zeeman effect, it is sometimes desirable to use the wave number corresponding to the frequency of the Larmor precession. This frequency may be designated by L, and the wave number L/c is then denoted by $\tilde{L}$. We have found this notation extremely convenient.

There is need for standardization of the symbol for Schrödinger's wave amplitude. We usually indicate this by the symbol Ψ. The wave amplitude referring to an individual quantum state of a system of one degree of freedom is indicated by Ψ_n, where n is the quantum number of this state, and then

$$\Psi_n = \psi_n \exp(-2\pi i E_n t/h)$$

where ψ_n indicates the part of Ψ_n depending on the coordinates. The choice of the minus sign in the exponential factor is dictated by the fact that we often use the transformation

[2] Evans and Burr, "A New Dietary Deficiency with Highly Purified Diets," *Proc. Soc. Exp. Biol. and Med.*, Vol. 24, 740 (1927); Vol. 25, 41 and 390 (1927–1928).

$$\Psi = \exp(2\pi i W/h)$$

W may be considered as a generalization of Hamilton's principal function, $-Et+S$, where S is the action. The writers feel that much good would come from the establishment of committees on mathematical notation in each of the national societies dealing with physical and chemical science, and trust that this note will evoke discussion directed toward this aim.

ARTHUR EDWARD RUARK
HAROLD C. UREY

MELLON INSTITUTE

MONOLITHIC SOIL PROFILES

EVER since the serious study of soils began, the preparation of portable monolithic soil profiles has been a time-consuming task. The department of agronomy of the North Dakota Agricultural College and Experiment Station has developed a rapid method of taking monolithic soil profiles to the depth of forty inches in stone-free soils.

Bore a hole forty-six inches deep with a nine-inch basket type post-hole auger. With a spade, dig a rectangular recess two to four inches in depth opposite the portion of the exposed soil to be sampled. After removing the débris from the hole, square the surface opposite the recess and place the open side of a rectangular trough of eighteen gauge galvanized steel two inches by four inches by 40 inches against the prepared surface. Place rigid backing against the steel trough to prevent its buckling under pressure. After placing footing in the recess introduce a short jack with three-foot handle (known as a balloon tire jack) between the footing and the trough and apply pressure near the bottom end of the trough and again near the top of the trough. If pressure is applied in excess of the pressure needed to fill the trough, layers develop in the soil at right angles to the direction of the pressure facilitating the removal of the filled trough. Placing the spade in the soil two or three inches from the open side of the trough pointing toward the trough and extending gentle pressure toward the excavation, the profile is easily broken away.

A monolithic profile may be taken from stone-free soils in one to one and a half hours. All students of soil phenomena will find this method worthy of trial.

A more complete description of this method and its adaptation to glacial soils will appear later.

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AN EXTINCT CAMEL FROM UTAH

IN SCIENCE for July 6 my friend Dr. Alfred S. Romer gave us an interesting account of the discovery of a skull of a camel, probably *Camelops hesternus*, in a dry lava-flow cavern near Fillmore, Utah. According to the statements furnished, the partial skull was buried at a depth of three or four feet in a fine eolian dust, about two hundred feet back from the mouth of the cavern. The bone had undergone no fossilization and there adhered to it a fragment of organic matter regarded as muscle. Dr. Romer holds that this discovery shows that this species lived in the west up to a relatively late time, only several centuries or at most several thousands of years ago. The present writer suggests that the recent deposits along the streams of the Great Basin be carefully scrutinized for additional remains of these modern camels.

Dr. Romer further concludes that my theories about the succession of Pleistocene vertebrates are thereby rendered much in need of revision. If I may judge from his suggestions that the fossils found at Lake Lahontan, Rancho La Brea and Frederick, Oklahoma, may be of a rather late date, his revision of Pleistocene history will restore the happy time when any species whatever of Pleistocene mammal might be expected to occur in any late Pleistocene deposit whatever, especially if its occurrence appeared to have any bearing on human history. A revision of that kind would evidently bring relief to some geologists and to many, if not to most, of our anthropological colleagues.

The determination of the geological age of the skull in question will turn upon two considerations. The first has regard to the length of time organic matter can endure in the conditions described by Dr. Romer. He grants that it might last indefinitely if bacterial action be prevented. Why not then a half million years? If the flesh has lasted until it has become covered with fine dust three or four feet thick, how are the destructive bacteria to reach it? Even if the dust were to be wet the bacteria would be filtered out before reaching the putrescible substance. Aqueous solutions of organic matter will endure indefinitely if bacteria are excluded. And probably that dust has never been wetted since it was deposited.

Dr. Romer is sure that the muscle could not have resisted decay for a half million years because that climate enjoys an annual rainfall of fifteen inches. From Bigbone Cave, Van Buren County, Tennessee, Dr. Harlan obtained megalonyx bones which, buried in fine dust, retained patches of cartilage, shreds of ligaments and pieces of the horny nails; and this was in a region where there is an annual rainfall of fifty or more inches.

The second consideration bearing on the age of the camel is the age of the cavern; and Dr. Romer, basing

his view on Gilbert's monograph on Lake Bonneville, appears to be certain that the cavern is a product of the Wisconsin glacial stage. Gilbert knew of only two glacial stages, but it is now believed that there were four or even five; and the Bonneville high water may have occurred during one of the earlier ones. The writer is of the opinion that it corresponds more nearly to the Aftonian interglacial stage, and the finding of the camel skull is evidence for this view. The cavern should be explored. It probably constituted the den of some ancient carnivore and the camel may be found to have been associated with other early Pleistocene remains, not with ranch cattle or even recent native mammals.

As a corollary of his determination of the age of the Bonneville beds Gilbert referred the Fossil Lake vertebrates to the latter half of the later glacial epoch. I shall be greatly interested in reading Dr. Romer's defense of that proposition. How will he account for a percentage of at least 50, perhaps of over 65, of extinct animals? Where in deposits overlying Wisconsin drift will he find such a high proportion of extinct forms? If he can support his thesis he will have four more camels to his credit. Furthermore, would not the climate at Fossil Lake during the Wisconsin stage have been rather cool for some of those camels, those peccaries and that ground-sloth? He appears to believe also that the fossils found at Frederick, Oklahoma, are of a late date. I shall be delighted to listen to his elucidation of the geology and paleontology of that region, where some hundreds of feet of deposit have been swept away since those animals lived there and here he will have to explain why all the species are extinct. He is mistaken in thinking that my study of the Iowa Pleistocene led me to believe that camels ceased to exist after the first glacial stage. It was there that I learned that they existed during that stage. Failure to find them in other deposits that could be demonstrated as of later age has been my reason for concluding that they did not long survive the first interglacial stage.

Furthermore, I do not rely on camels alone for my views of Pleistocene history. There are probably fifty species of important vertebrates with which camel remains are commonly associated which appear to have become extinct at the same time. If Dr. Romer holds the opinion that all the fossil vertebrates found at the localities he mentions lived only a few thousand years ago, will he not tell us what kinds of animals lived during the early Pleistocene and where their remains have been collected?

The writer will continue to hope that the geologists, the paleontologists and the anthropologists who do not like his opinions on Pleistocene geology, paleontology and anthropology will speedily collaborate and impart to us their conclusions and their reasons therefor.

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CENTERS OF RESEARCH

Under the above title, Professor S. R. Williams, in Science for July 20, deplores "the paucity and mediocrity of the research produced by some of the graduate schools of our large universities," and suggests that the remedy for this deplorable condition is to centralize the direction of research in each department under one man, permitting, indeed, the other members of the staff to carry on their own research if time and circumstances permit, but denying them the privilege and stimulus of directing students in research.

With Professor Williams's judgment that much that passes for research does not measure up to the standards of good scholarship there will be little disagreement. But his analysis of the cause of this condition is far from convincing, and from his proposed remedy there should be sharp dissent. He suggests that if one were to set down a list of research centers and classify them according to whether the work sent out is done under the leadership of one man or of several, the results will be surprising. I have tried this for the field of botany, and I am sure that the results would be surprising to Professor Williams. Such a list must, of course, be very largely a matter of personal opinion, but personal opinion is scarcely sufficient to justify a proposal for a radical change in the organization of American universities. It is at least probable that the underlying difficulties with American scholarship are to be sought for in social and economic conditions far more fundamental than matters of departmental organization, and there is no convincing evidence that the substitution of dictatorships for such measure of democracy as we now enjoy would materially remedy the situation. This is not to deny that great teachers and investigators may and should dominate an individual department. They have done so in the past by virtue of their own innate qualities, and there is no reason to doubt that they will continue to do so on the same grounds. But that is a very different thing from a proposal to centralize such power in men who, even if they be good investigators, may be narrow-minded, selfish or autocratic. Even assuming that such a policy of intellectual fascism might result in certain local gains in efficiency, it is difficult to see how the loss of freedom and initiative on the part of the great mass of subordinate workers could result in anything but a further lowering of the general standard of scholarship.

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QUOTATIONS

DARWIN AND DOWNE HOUSE

DOWNE HOUSE, in which Charles Darwin lived for nearly forty years, is now a gift to the nation, entrusted to the British Association for the Advancement of Science. More than a quarter of a century ago Andrew Carnegie thought of buying it and putting up a sum of money to settle, as he phrased it, one way or another, the question of evolution. But those whom he consulted felt bound to advise him that, as a business proposition, the idea was unsound. Later on Sir Arthur Shipley, Master of Christ's College, Cambridge, where Darwin had passed his undergraduate career, urged that some way should be found of making Downe House a national possession. But the times were unpropitious. A few years ago Professor H. F. Osborn, of New York, again propounded a scheme for transforming Darwin's home into an endowed center for evolutionary research, and suggested that part of the funds might be supplied from America if the Royal Society would adopt and develop the idea. The council of the society, after friendly and detailed consideration, came to the conclusion that a very large sum of money would be required to transform a comparatively small country house into a research institution and to provide for its staff and maintenance. Even if the sum were available, it could be spent to greater scientific advantage in the development of some of the existing research institutions. At Leeds last year Sir Arthur Keith, then president of the British Association, issued an appeal, with the authority of the council, for the more modest object of preserving Downe House simply as a memorial of England's greatest naturalist.

The appeal had a swift and fortunate response, for Mr. Buckston Browne, a distinguished London surgeon, offered to buy the house, provide funds for its maintenance and make it a gift to the nation in the custody of the British Association. With the generous cooperation of the Darwin family, the end has been achieved and the house and the eighteen acres in which it stands are now vested in the British Association. Some of the actual pieces of furniture used by Darwin in his study have been presented by the family, and Mr. Buckston Browne is collecting other pieces of the same period so as to reproduce as closely as possible the actual environment in which "The Origin of Species" and many other great books were written. All the editions of Darwin's books are being got together, and as soon as the lease can be acquired from the present tenants Downe House will be opened to the public. There is a superstition that the aura of evil deeds lingers in the premises in which they were committed. If there be no supernormal vestige left by great men, at least our imagination is quickened and our sympathies attuned to gracious memories by seeing the simple surroundings in which they thought and worked. The rooms in which Darwin wrote, the garden paths on which he paced, and the simple greenhouses in which he conducted his experiments, if only because they are homely and undramatic, can make us realize the possibilities of human achievement. For there patience and genius, the most faithful devotion to pedestrian fact, and the most daring imagination combined to bring about a stupendous revolution in human thought. Access to Downe House will preserve for all time the inspiring personality of the man who, in the words of Mr. Punch's inspired epitaph, was

> Recorder of the long Descent of Man,
> And a most living witness of his rise.
>
> —*The London Times.*

A RELATION BETWEEN THE INCIDENCE OF COMMON COLDS AND NUTRITIONAL HYDRATION

THE subject of colds has been of personal interest for over twenty-five years, inasmuch as it was noticed in early youth that I seemed to catch colds more frequently than others in my environment. The hypersusceptibility probably has a constitutional or hereditary basis. However, the rôle of nutritional factors has been emphasized in the last twenty years, as colds never developed during periods of experimental fasting or undernutrition. The fasts of from one to forty-one consecutive days now total over five hundred days. Periods of undernutrition have been longer. Colds never developed during the fasts, although I then often felt extremely uncomfortable upon exposure to cold or cool drafts—evidently because of the lowered heat production at such times. But a fact for which no explanation suggested itself until about a year ago was the almost invariable development of colds shortly after the prolonged fasts. That is, these colds came on with the liberal post-fasting feeding, but before the pre-fasting nutritional level was restored. Moreover, they developed when the general state of well-being seemed to be better than the pre-fasting physical state.

An explanation for this peculiar susceptibility to colds after fasting was suggested by the finding that the colds were caught when post-fasting edema or hydration was most prominent. Some edema was observed after all my longer fasts and also after

periods of undernutrition. It has been observed in others who have fasted and explains the common rapid post-fasting increase in weight which can not be attributed to the food intake alone. It seems to be similar to the "war edema," "jail edema," "epidemic edema" or "nutritional edema" which have been reported from time to time. It tends to clear up with an adequate food (particularly protein) intake but persists in milder form under some circumstances. In my experience it began promptly with a slight ascites, and subcutaneous or generalized edema became evident a few days later. The greatest degree of hydration was manifested by about the fifth day after the close of the last two prolonged fasts of thirty-three and forty-one days, respectively.

Some data regarding the state of hydration before, during and after the thirty-three-day fast were obtained with the intradermal salt solution test of McClure and Aldrich,[1] by Kunde.[2] Further data were secured in connection with the forty-one-day fast. The disappearance time of the wheals, in otherwise normal skin areas, decreased from about sixty minutes before, to fifteen minutes a few days after, fasting. At the ankles, which have been affected by a circulatory deficiency (varicose veins in the legs), the disappearance time decreased from about twenty minutes before fasting to less than one minute at the peak of hydration. The disappearance time returned to normal within a month after the thirty-three-day fast, which was followed immediately with a relatively high protein intake, but the test showed edema for at least three months after the forty-one-day fast, which was followed by sixteen days of protein restriction. This test was not used longer because, among other things, it did not seem to yield better data regarding variations in hydration than close observation of weight fluctuations and of changes in degrees of swelling and pitting. Nevertheless, it is noteworthy that I caught one cold after another as long as the test of McClure and Aldrich indicated some edema.

The liberal use of protein immediately after fasting seems to mitigate the edema, but I have never succeeded in thus preventing it. After post-fasting edema is well established, the excessive use of protein or of other food in addition to sufficient protein appears to prevent it from clearing up. I have never tried a purely carnivorous diet or carbohydrate restriction immediately after prolonged fasting. However, under more normal circumstances, I had two experiences of about ten and three weeks, respectively, with meat alone. On these occasions a slight previous edema decreased. The intradermal salt solution test also showed decreasing hydration *during* the course of the forty-one-day fast. This finding is contrary to the reports of increased hydration of the organism during starvation, but it should be borne in mind that the test of McClure and Aldrich reflects only local conditions directly and the hydration of fasting is probably not an edema. But here it is of interest to note that with a carnivorous regimen as well as after the first few days of a prolonged fast one lives practically on proteins and fats alone. Nevertheless, distinct edema did not follow the carnivorous diet periods as it followed fasting. This appears to show that the carbohydrate starvation during fasting was not responsible for the post-fasting edema and it also suggests that the prevention of protein starvation protects one from such edema. This view is also supported by the work of Kohman.[3]

However, I caught a cold at the beginning of my first carnivorous diet experiment. This cleared up quickly, although the amount of meat eaten was increased to an average of about four pounds (1.8 kg.) daily. This cold may have been the consequence of the preceding mixed diet, but an attempt to confine myself as much as possible to lean meat may also have been responsible. As Stefánsson has repeatedly stated,[4] a carnivorous diet without a fair proportion of fat is not well tolerated by man. In a study of the effect of variations in the protein intake on gastric acidity,[5] I also found evidence of a limit to the amount of protein that could be used. But bearing more directly on the present problem is the fact that an excess of protein serves as a source of carbohydrate and nitrogenous waste products which may be stored or retained with a considerable amount of water. In a second experiment with a carnivorous diet, I aimed to use as large a proportion of fat as possible. Nevertheless, I found it necessary to take more protein to satisfy myself than with a mixed diet under conditions otherwise similar. Evidently the protein intake must be sufficient to replace losses due to tissue breakdown and also to furnish the carbohydrate fraction which is necessary to burn the fat and prevent ketosis. But I was particularly impressed, during this period, that I became freer than usual from a persistent nasal catarrh, which was often evident between colds.

On the other hand, I tried vegetarian diets and followed the low protein idea of Horace Fletcher,

[1] McClure, W. B., and C. A. Aldrich, *Jour Amer. Med. Assn.*, lxxxi, 293, 1923.

[2] Kunde, M. M., *Arch. Int. Med.*, xxxviii, 57, 1926.

[3] Kohman, E., *Amer. Jour. Physiol.*, li, 378, 1920.

[4] Stefánsson, V., "My Life with the Eskimo," New York, 1913; "The Friendly Arctic," New York, 1921.

[5] Hoelzel, F., *Amer. Jour. Physiol.*, lxxvii, 166, 1926.

Chittenden and Hindhede for nearly ten years without noting any decrease in the tendency to catch colds unless the protein restriction amounted to protein starvation. Then the addition of salt to the diet and occasional increases in the protein intake (due to instinctive compulsion) gave rise to considerable edema. Furthermore, the susceptibility to colds was increased and pleurisy developed under such circumstances in the winter of 1917–1918. During the next year, as a result of a change in diet occasioned by military service, I found that the continued use of a higher protein intake decreased the tendency to develop distinct edema from additions of salt or more protein. However, the army diet did not confer immunity to colds, nor did evidence of slight edema decrease to the extent that it decreased later with the carnivorous diets or with a low carbohydrate diet such as I have adopted more recently to control both hydration and the susceptibility to colds.

The restriction of the carbohydrate intake as a means of keeping the hydration of the organism at a low level is based upon the fairly well-established fact that along with carbohydrate a considerable amount of water is stored. At least, with the carbohydrate reserve kept at a relatively low level, I find that ordinary colds do not develop. A limitation to about five hundred calories from carbohydrates in a diet of about twenty-five hundred calories daily seems satisfactory. The protein intake has been kept adequate (1.2 to 1.6 gm. per kg. of body weight) and the balance has been made up with fats. With this regimen, I have caught no distinct cold but have noticed a little sneezing from time to time, or an excessive secretion of nasal or pharyngeal mucus which generally cleared up within a few hours without anything being done about it.

It may seem idle to speculate at this time about the mechanism of catching a cold. But presumably, with the organism hydrated to a high degree and with the tissues in general already under some degree of tension, a sudden cooling of the skin may throw an overload of fluid on internal structures, including the upper respiratory tract. Undoubtedly the process is not simple. A specific nervous or vasomotor reaction seems to be involved in the chill which often initiates a cold. A factor in the development of this reaction may be an increased sensibility of the cutaneous nerves in nutritional edema.

Although quantitative data on this subject were not secured, I have repeatedly observed the variations in skin sensibility with changes in the degree of hydration. In shaving, the "pulling" due to a razor which is not very sharp is far more painful with edema than under normal conditions. A difference was often seen here between morning and evening, as the hydration of the face which is greatest in the morning (after reclining) decreases during the day with the maintenance of the upright position. This change may, however, become reversed if the diet used during the day leads to a considerable retention of fluid. Pinching of the skin has shown similar fluctuations in sensibility in other skin areas. This has been so constant that I am inclined to regard the return of normal sensibility as a better index of the complete disappearance of edema than other tests. In using the edema test of McClure and Aldrich, the pain incident to puncture with the needle was also greatest with the most pronounced edema. This discouraged the use of the method. The change in sensibility may be qualitative rather than quantitative, as the pain in edema reminds one of the pain of mild inflammation. The theory that there is a nervous factor in the development of edema is therefore hereby supported.

The above explanations would harmonize the theories that refer colds to dietary excesses with those that refer them to changes in the weather or to exposure. The oft-quoted freedom from colds of Arctic explorers would be regarded as the consequence of a decreased carbohydrate intake rather than of outdoor life or relative absence of bacteria. The effectiveness of remedies, such as food restriction, sweating and bed rest would be explainable as being due to dehydration. However, it is hard to understand why sodium bicarbonate should be beneficial, as Cheney[6] recently reported. Perhaps the other measures he uses are more directly responsible for the results. Cheney's implication of the protein intake as a cause of colds is supported if it is remembered that this is a protein excess in a diet otherwise excessive.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

PARAFFIN SECTIONS OF TISSUE SUPRAVITALLY STAINED

THE use of Nile-blue sulphate for staining living Amblystoma embryos which will furnish grafts for transplantation in the study of many problems in embryonic development has been popular for some time (Detwiler, 17).[1] Its main drawback has been that

[6] Cheney, V. S., *Amer. Jour. Pub. Health,* xviii, 15, 1928.

[1] S. R. Detwiler, "On the Use of Nile-blue Sulphate in Embryonic Tissue Transplantation," *Anat. Rec.*, V. 13, 493–497, 1917.

the color of the grafted tissue remains only while the embryos are alive and that the limits of the transplanted cells can no longer be traced by the stain in embryos which have been fixed and passed through alcohols. During the course of blood work in which supravital technique was employed, Miss Fazilé Shevket noted a method described by McJunkin (25)[2] for the examination in paraffin sections of tissue supravitally stained. This method with slight modifications was tried out by Miss Kathryn Stein on Amblystoma embryos stained in Nile-blue sulphate and gave good results. Because it may be of value to other workers in this field, the method, quoted from McJunkin (pp. 313–314), is given below.

The tissue stained supravitally in toto is fixed for twelve hours in a Zenker-formol solution, consisting of 15 cc of 40 per cent. formaldehyde and 85 cc of Zenker's fluid without acetic acid. It is then cut into pieces not to exceed 3 mm in thickness and transferred to Zenker's fluid without acetic acid for 12 to 25 hours. Bits of the tissue are then placed in pure absolute acetone for 1 hour (2 changes), in benzol for 20 minutes and in 52° C. paraffin for 20 minutes. Sections of the desired thickness are cut and attached to the albumin-coated slides by allowing them to dry overnight at room temperature. To stain, the paraffin is removed with xylol (10 seconds) and pure acetone (10 seconds). After immersion in water (5 seconds) the slide is stained very lightly with hematoxylin (Harris without acetic acid) for about 5 seconds and immersed in tap water (5 seconds). The section is then dehydrated with pure absolute acetone for 10 seconds, at once immersed in xylol for 10 seconds and mounted in balsam. To limit the action of the acetone and xylol to these times the slides are stained singly and the solutions run over them from a dropping bottle. A 0.05 per cent. aqueous solution of methylene blue may be substituted for the hematoxylin.

This method has been used on Amblystoma embryos up to stage 40 and the stained tissue showed clearly. There appears to be no reason for its not being applicable to older embryos provided the color is still visible in the living animal. The staining with Nile-blue sulphate was done in the usual way (Detwiler, 17)[1] and the embryos to be examined were fixed according to McJunkin's scheme and sectioned. It was found that they could remain in acetone longer than one hour without harm and that xylol could be substituted for benzol. An hour for infiltration did not appear to be detrimental. It also seemed to be unnecessary to stain the sections with hematoxylin unless the nuclei were to be examined particularly because the outlines of the vitally stained tissue were evident in the sections after mounting.

[2] F. A. McJunkin, "The Origin of the Mononuclear Phagocytes of Peritoneal Exudates," *Amer. Jour. Path.*, V. 1, 305–324, 1925.

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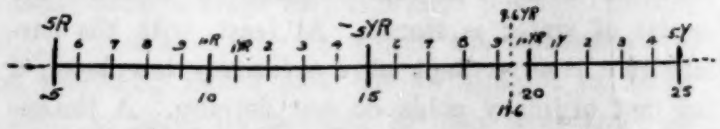
COLOR DISCS IN SOIL ANALYSIS

IN a note[1] regarding color discs used in soil analysis, G. B. Bodman suggests that a ratio, $\frac{\text{"Red"}}{\text{"Yellow"}}$, as determined by color disc analysis is found to bring out the apparent striking color differences of two soils.

The ratio, $\frac{R}{R+Y}$, the proportion of R to the total color, will give better results, while the use of the notation of the Munsell color discs in the above ratio will give still better results in that it expresses the hue as the eye sees it. For example, the red disc being used in soil work is R 4/10, and the yellow, Y 8/9. The first figure in the notation represents the brilliance of color on a scale of 0 to 10, black to white, the second figure chroma, or color saturation, 0 being neutral, 10 expressing strong chroma.[2]



By translating the hue letters R and Y to figures, *i.e.*, R = 5, Y = 25 (the hue circle being completed in 100 steps, YR = 15, GY = 35, G = 45, BG = 55, B = 65, PB = 75, P = 85, RP = 95, back to 5R), and letting x equal the first hue (clockwise), while z equals the second, use the formula (which is essentially the same as $\frac{R}{R+Y}$):

$$z - \frac{S(A_1 \cdot B_1 \cdot C_1)}{S(A_1 \cdot B_1 \cdot C_1) + S(A_2 \cdot B_2 \cdot C_2)}(z - x),$$

when A = per cent. area, B = brillance, and C = chroma. The first area, A_1, applies to the hues taken in a clockwise direction, that is, to red, while A_2 applies to yellow. S is the summation sign.

For example, if the percentages matching a certain soil color are 10 per cent. red, 15 per cent. yellow, 70 per cent. black, and 5 per cent. white, when the Munsell notation for red is R 4/10, and that of yellow, Y 8/9, the relative hue may be found by substituting in the formula above:

$$25 - \frac{(10 \cdot 4 \cdot 10)}{(10 \cdot 4 \cdot 10) + (15 \cdot 8 \cdot 9)}(25 - 5) = 19.60.$$

[1] SCIENCE, April 27, 1928, p. 446.

[2] *Cf.* Colorimetry Report of the Optical Society of America, 1920–21, *J. O. S. A.* and *R. S. I.*, Vol. VI, No. 6, August, 1922, pp. 534–5 for definition of hue, brilliance and chroma.

Just as 5=5R and 25=5Y, so 19.60=9.6 YR. See diagram, and refer to "A Practical Description of the Munsell System," by T. M. Cleland, for information concerning the Munsell notation.

The formula given will hold as long as the hues of the discs used are the same through any experiment, and as long as they are fairly close in hue relationship.

The ratio given for reflection as $\frac{I\,6500}{I\,5000}$ may also be improved by using $\sqrt{\frac{S\,(A \times B^2{}_{1.2.3\ \ldots})}{100}}$.

The value, or brilliance scale, of the Munsell color discs follows approximately the square root of the reflection, that is, on a scale of grays 0 to 10, black to white, middle gray, or 5, reflects about 25 per cent. of the light falling on it, while the seventh gray step will reflect about 49 per cent. of light.

In the example given for hue we might work out the brilliance as follows (when white is N(eutral) 9/ , and black N 1/.):

$$\sqrt{\frac{(10 \times 4^2) + (15 \times 8^2) + (70 \times 1^2) + (5 \times 9^2)}{100}} = 3.99.$$

The brilliance, therefore, is 3.99 on a scale of equidistant steps from 0 to 10, black to white.

This formula, like the hue formula, may be used with confidence as long as the discs used in an experiment are of the same color. It will be closely approximate when other discs are used.

This material is passed on for the information of those who are using Munsell discs in their colorimetric work. Few people, although they use the papers, seem to realize the advantages that the Munsell notation offers for specifying and studying their color work.

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SPECIAL ARTICLES

THE STRUCTURE OF BORON HYDRIDES

IF the idea of the pseudo-atom[1] is extended to the hydrides of boron it is possible to write formulae for these substances that conform more nearly to our usual notion of molecular structure than do formulae advanced up to date. It is not necessary to involve the K electrons[2] of the boron atom in the building of a complete octet of L-electrons, nor to assume boron tetra or pentavalent,[3] nor need four electron bonds be assumed.[4] In fact, on the scheme given below a whole boron chemistry can be conceived of in complete analogy to carbon chemistry.

Atom	Pseudo atoms in carbon chemistry	Pseudo atoms in boron chemistry
=C–	=C=	=BH=
=N–	=CH–	$=BH_2$–
–O–	$-CH_2$–	$-BH_3$–
F–	CH_3–	BH_4–

For example, the hydrides with two central atoms (either two carbon or two boron atoms) have on this theory the following structures:

HYDROCARBONS

Ethane	CH_3–CH_3	C_nH_{2n+2}
Ethylene	CH_2=CH_2	C_nH_{2n}
Acetylene	CH=CH	C_nH_{2n-2}

HYDROBORONS

.....	BH_4–BH_4	B_nH_{3n+2}
Borethane	BH_3=BH_3	B_nH_{3n}
.....	BH_2=BH_2	B_nH_{3n-2}

While Stock[5] compares B_2H_6 to ethane and assumes boron tetravalent it seems equally reasonable to make comparison with ethylene, as the following figures show:

COMPARISON OF PHYSICAL PROPERTIES

	Melting point (°C)	Boiling point (°C)
C_2H_6	–172.0	–88.3
B_2H_6	–166.0	–92.0
C_2H_4	–169.4	–103.8

As far as the chemical behavior is concerned it is to be pointed out that the structure proposed for B_2H_6 differs from ethylene in the complexity of the positive change within an octet and so a difference in chemical behavior may be explained on this basis. That is, if the positions of the positive charges (B^{+++}, H^+, H^+ and H^+) within the octet are not further specified, then the bond holding the two radicals

[1] H. G. Grimm, *Z. f. El. Chem.*, 31, 475, (1925).

[2] E. D. Eastman, *J. Am. Chem. Soc.*, 44, 438–51 (1922) and E. Miller, *Z. f. El. Chem.*, 31, 382–5 (1925).

[3] A. Stock, *Ber.*, 59, 2223–6 (1926) and J. A. Christiansen, *Z. f. allg. u. anorg. Chem.*, 160, 395–403 (1927) and J. Böeseken, *Ber.*, 58 (1) 268–70 (1925).

[4] M. Huggins, *J. Phys. Chem.*, 26, 833–5 (1922).

[5] A. Stock and Pohland, *Ber.*, 59, 2223–6 (1926).

BH_3 together is a (double) bond, as in ethylene or oxygen. The difference in chemical behavior of O_2, C_2H_4 and B_2H_6 must then be ascribed to the fact that in these three cases the positive charge within an octet consists of a single kernel in O_2, a carbon kernel and two protons in C_2H_4 and a boron kernel and three protons in B_2H_6.

The next step would be to locate the protons within the octet in some symmetrical fashion, but such a detailed picture is at present not necessary.

It is evident that the present view considers the completion of a stable shell (an octet) as the primary cause of chemical combination and the valency of an individual atom as a rather secondary phenomenon.

In the first instance, it is only necessary to consider the radicals BH_3, BH_2, etc., equivalent to CH_2, CH, etc. In both cases their positive charges are thought to be located inside of an octet of electrons. Further details of this theory will be published in another place.

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ON THE SPECTRUM OF DOUBLY IONIZED MAGNESIUM

AN investigation of the condensed spark discharge in vacuum between electrodes of pure metallic magnesium has revealed the levels of the most stable three configurations of the doubly ionized atom, isoelectronic with neon.

The transition frequencies and term separations were predicted to a high degree of accuracy by the X-ray doublet laws and a consideration of the first order screening numbers; the values were extrapolated from the arc spectrum of neon, which is well known from Paschen's famous analysis, and the first spark spectrum of sodium, recently analyzed independently by four different investigators (Bowen, *Phys. Rev.*, 31, 967, 1928; Frisch, *Zs. f. Phys.*, 49, 52, 1928; Laporte, *Nature*, 121, 941, 1928; Majamdar, *Ind. Jour. Phys.*, 2, 345, 1928).

The notation of the s– and p– terms follows that used by Paschen in neon. Several possible values have to be listed for the p– terms with inner quantum numbers 3 and 0; we hope that values for these terms may be selected unambiguously later by combinations with higher terms.

The key to the analysis was found in the resonance lines, $\lambda = 231.71$, $\nu = 431570$, int. = 2, and $\lambda = 234.25$, $\nu = 426890$, int. = 1, the stronger of which was recorded by Millikan and Bowen in their first report on extreme ultra-violet spectra (*Phys. Rev.*, 23, 1, 1924). All the other lines are new. All the expected transitions between the terms listed have been found, with the exception of s_2–p_{10}, $\lambda_{calc} = 2881.2$, which lay outside the range investigated, and $s_4 - p_7$ and $s_4 - p_9$ whose intensity was expected to be low.

The terms of Mg III have the following relative values:

configuration	name	j-value	term value
$2p^6$	^1S	0	–425680
$2p^5 3s$	s_5	2	0
	s_4	1	1216
	s_3	0	2193
	s_2	1	5881
$2p^5 3p$	p_{10}	1	40589
	p_9	3	47906
			or 48480
			or 48625
	p_8	2	48888
	p_7	1	49822
	p_6	2	51935
	p_5	1	51980
	p_4	2	52270
	p_3	0	52328
			or 52780
	p_2	1	54608
	p_1	0	57290
			or 58432

The ionization potentials of magnesium are as follows:

Mg I	$3s^2$	$^1S_0$	to Mg II	$3s$	$^2S_1$,	7.61	volts
Mg II	$2p^6 3s$	$^2S_1$	to Mg III	$2p^6$	$^1S_0$,	14.97	volts
Mg III	$2p^6$	$^1S_0$	to Mg IV	$2p^5$	$^2P_2$,	80 ± 2	volts

Most interesting in the neon-like isoelectronic sequences is the trend of the relative separations in the $2p^5 3s$ "triplet" $s_5(^3P_2)$, $s_4(^3P_1)$, $s_3(^3P_0)$. The relative separations $^3P_1 - {}^3P_2$: $^3P_0 - {}^3P_2$ are as follows:

"normal"	.667
Ne I	.537
Na II	.564
Mg III	.554

The downward curvature of this ratio might be taken as evidence against Hund's correlation of inverted terms to series limits. (See Mack, Laporte, and Lang, *Phys. Rev.*, 31, 748, 1928, esp. p. 763 for the analogous case of the d^9s triplets, and Shenstone, *Nature*, 121, 619, 1928, for a more general discussion of series limit correlation of inverted terms.)

A more complete report on the spectrum of Mg III will be published after a search for higher terms has been completed.

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